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a record of research

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THE EVOLUTION OF MAN'S CAPACITY FOR CULTURE

The first six essays of this issue of *Human Biology* constitute a symposium, "The Evolution of Man's Capacity for Culture," presented at the annual meeting of the American Anthropological Association, Chicago, Illinois, on December 29, 1957.

The symposium, with a preface by Dr. J. N. Spuhler of the Department of Anthropology, University of Michigan, who organized it, and a summary review by Professor Leslie A. White, of the same department, will be reissued this spring as a book by Wayne State University Press, Detroit (price about $3.50).

In concluding his summary, Professor White says: "This symposium goes considerably beyond the scope indicated by its title. It not only outlines the course of biological development that eventually produced man's bodily structure; it shows how primate behavior of various kinds, but particularly the use of tools, has profoundly affected the evolution of man's body and brain. The carry-over of certain trends and tendencies in prehuman primate behavior, both social and individual, and their expression in human behavior and culture, has been much discussed in the past, and this topic is competently treated in this symposium. But a point much more rarely considered is that which sees the emergence and establishment of culture opposing some fundamental traits of primate nature and practise. This, also, is illuminatingly treated. And in addition to ideas, views, and interpretations, the various papers have gathered together and presented a wealth of factual information from far and near and offer it here for the reader in succinct, compact form."

The six authors view man's evolving capacity from six directions: in front, behind, above, below and both sides; that is, from the points of view of anatomy, physiology, psychology, language, culture, and society.

GABRIEL W. LASKER, *Editor*
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SOMATIC PATHS TO CULTURE

BY J. N. SPUHLER[1]
University of Michigan

FOR the present discussion, I am going to assume that culture is a biological adaptation, with non-genetic modes of transmission, which greatly supplements somatic evolution. Viewed in this way, there is a gap between cultural behavior and non-cultural behavior. The two sides of the gap are defined in terms of symbol and lack of symbol. Also, viewed in this way, we see that the gap is bridged. The gap was crossed in the past by the human species, and it is still being crossed by babies as they learn to become human.

Now certainly the behavior we observe in all human societies is fundamentally different in some respects from the behavior we observe in societies of monkeys, apes, and insects. We all recognize the rich symbolic character of human behavior. But I want to stress the bridge—the crossing of the gap from non-symbolic to symbolic behavior—and not the gap itself. To me it appears absolutely necessary to consider both in the history of the species and of the individual that certain conditions are prerequisites for the full acquisition of culture whether by individuals or by the species. And we find these elements in babies before they begin to use symbols and we find certain of them in the behavior of monkeys and apes. Of course the above statements refer to culture in the specific sense of that variety of culture realized by members of the genus *Homo* and not to culture in the generic sense, not to all conceivable varieties of culture.[2]

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[1] Aside from minor changes, the present paper is the version presented in the American Anthropological Association symposium on the "Evolution of Man's Capacity for Culture," Chicago, 29 December 1957.

Since all agree that modern man has culture, or is cultural, the easiest way in which I could discuss the morphological paths to culture would be to summarize the evidence which supports some particular phylogeny leading to man. The argument would then be: 1) Man has culture. 2) This or that phylogenetic diagram tells us how man changed from some non-cultural stem primate in the Paleocene to what he looks like today. In one sense this would be a proper discussion of the somatic paths to culture. It is important, and perhaps lucky, that we can make such pictures of man's biological history with a considerable amount of credibility. But this approach, while valuable, in not sufficient to help us understand the historical biology of human behavior, and I am not going to give it much attention here.

In thinking about human phylogeny, I believe in using all, or nearly all, the hominoid fossils we know about, so long as they are not fragments. To argue that none of the known man-like fossils are in *the* human phylogenetic line seems to me obscurantist. To argue that the fossils we know about are "somewhat near" but not exactly on the main line seems unnecessarily cautious and hedging and may give the unknown greater weight than the known. Perhaps Weidenreich (1946) and Heberer (1950) went too far in using all known hominid specimens they considered authentic. But I prefer their use of all of them to Wood Jones' (1948) use of almost none.

There is not space here to give a review of new developments in human paleontology. In the last few years we have acquired a wealth of new specimens and new ideas and we have also been able to discard some old specimens and ideas with good cause. If additional fossils become available it may be necessary to make major revisions in what I am about to say. By taking an abstract level—the level of the taxonomic genus—I can avoid some undecided issues on the phylogenetic placement of individual specimens. For the moment I am going to assume a human evolutionary sequence of 4 or 5 genera (Clark, 1955):

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[2] In a more extended treatment it would be useful to make "society" a level of integration between biology and culture. There are a number of recognizable and important primate "social paths to culture."

1) Leaving out the periods before the Miocene, we start with *Proconsul*, the earliest ape whose skull is known (Clark and Leakey, 1951). I assume *Proconsul* had precursors who developed the general features of a man-like thorax and arms as we know them today, but that these terrestrial apes were not highly specialized as brachiators. There is no reason to suppose that any human ancestors since the Miocene have been arboreal to the extent characteristic of living gibbons, orangutans, or chimpanzees (Leakey, 1952; Washburn, 1951).

2) We don't know what happened in the Pliocene.

3) At least by Early Pleistocene there is *Australopithecus*, now known from dozens of good, or as Broom would say, "beautiful," specimens, and the earliest evidence of man-like animals with bipedal locomotion.

4) By Early Pleistocene times, and lasting into Middle Pleistocene in parts of Asia, we have the genus *Pithecanthropus*. From the neck down they were very like the genus *Homo* and like him they were tool makes, fire users, and hunters. Their brain volume was intermediate between *Australopithecus* and *Homo*.

5) At least by Middle Pleistocene we have the genus *Homo*, represented by such forms as Swanscombe, Fontéchevade, the Neanderthals, and Upper Paleolithic man. Everyone agrees that some, if not all, members of the genus *Homo* have culture.

Now, in the context of this sequence of 4 known genera, and with comparisons from living monkeys and apes, I want to discuss 7 biological topics which are preconditions for the beginning of culture. They are:

1) Accomodative vision,
2) Bipedal locomotion,
3) Manipulation,
4) Carnivorous-omnivorous diet,
5) Cortical control of sexual behavior,
6) Vocal communication,
7) Expansion of the association areas in the cerebal cortex.

Of course, these 7 conditions alone did not make a population of apes lacking culture into a population of men with culture. The evolution of man was not predetermined by a few conditions in a population of Miocene apes. Mutations are the fundamental genetic events in the historical process of the acquisition of the capacity for culture. Mutations are random events that do not point in an orthogenetic direction. But mutations are limited by the structure of the gene which mutates and this structure is determined by the evolutionary forces, especially selection, active in the history of the gene. In this way populations that survive accumulate genes which are favorable in the prevailing environment of the population.

To illustrate the complexity of human evolution since the Miocene as seen at the mutational level, let me do some speculative arithmetic—using figures that have fair justification and are conservative (Spuhler, 1948, 1956; Simpson, 1953). From the Miocene to now there must have been at least two million generations in the hominoid line. If the total breeding population in successful phyla was 10 thousand, we have 20 billion individuals as real or potential ancestors of modern man. If genes at the average locus mutate at a rate of 1 in 100,000, and if only 1 in 200,000 of these result in new and favorable steps (and that is a low estimate), we still could have about 20 thousand "visible," favorable mutational steps (in all loci) since the Miocene in the hominoid line.

Thus when we talk about 7 conditions we are perhaps oversimplifying the matter. But there is not time for further discussion, even if we knew what to say. And, I should add, the 7 conditions I list do not represent unit mutations, although mutation is the ultimate source of the genetic variation in each condition. Further, the order of listing is not strictly chronological. Evolutionary changes in the 7 conditions were interdependent and roughly synchronous.

1) *Accomodative vision.* Vision has been the primary sense in vertebrates as far back as we know them (Polyak, 1957). It makes possible their great mobility. The most complex vertebrates, birds and mammals, interact with their external environment predominantly *via* their eyes. Under the influence of the arboreal habitat, primate vision was perfected into a leading sense. Visual behavior is one key difference between the nocturnal, mostly solitary Prosimians, and the diurnal, more social Anthropoidea. The difference between these two is perhaps the largest gap in non-human Primate social behavior. With upright, or sitting-up posture, vision in the Anthropoidea gained strict control of

manipulation—it became super*vision*, a guide and control of fine manipulation.

The relationship between the evolution of keen vision and fine manipulation is two-directional. As Polyak (1957) says: ". . . vision itself [became] more refined and the intellectual absorption and mental utilization more complete and lasting, as the skilled movements became more complex and more efficient." We will find that this kind of both-way causation with two or more systems evolving simultaneously, where progress in each stimulates change in the other, is important to the understanding of many topics in this symposium.

Before taking up bipedal locomotion, let me mention one good thing that came out of the Piltdown affair. It was the insight given, for example in Hooton's excellent paper of 1925, on the asymmetrical character of human evolution. Hooton was right, mostly for the wrong reason (Piltdown), but he was early to stress that different regions of the human body change at different rates. Many workers today would follow Washburn's (1951) separation of the human body into three regions distinct in phylogeny, with arms and thorax the oldest, the bipedal complex of pelvis and legs later, and the head and face latest of all to reach their modern form.

2) *Bipedal locomotion.* Although functional differentiation of the front and hind limbs started with the first tetrapods where the front legs reach out and the hind legs push, *Australopithecus* is the first primate with upright bipedal locomotion (the tarsiers are bipedal hoppers). The australopithecine pelvis, sacrum, and femur resemble modern man in those features which make his upright posture possible. There are some features of full bipedalism not found in *Australopithecus*—these are fully developed in *Pithecanthropus* from Java and Peking. Australopithecine locomotion was certainly more similar to that of *Pithecanthropus* and *Homo* than to any of the quadramanus primates. We must conclude that, by the early Pleistocene, hominoids were bipedal with free hands which could be used to handle tools. We will see that this was a master adaptation that demanded other adaptations leading to man's capacity for culture.

3) *Manipulation.* A good start toward precise manipulation is seen in monkeys. When monkeys sit up their hands are temporarilly free and are used to bring objects close to the organs of touch, vision, taste, and smell. But something like a quantum jump is made when the hands

are continually free for such activity as they are in an upright, fully bipedal hominoid. Then the arms and hands—under the guidance of binocular vision with good accommodation—are principal organs for interaction with the immediate physical environment. Getting food, eating, grooming, fighting, making, using, and carrying tools, these manipulations, accompanied by a rich flow of sense data including those from the more developed proprioceptive arm-and-hand muscle sense, enlarge the flow of information to the brain which in turn fosters development of association areas for storage of past experience with the hands and guides and initiates new hand movements. The neural delay required when some extra-organic tool is interposed between stimulus and response probably had much to do with the first ability to use symbols and the start of language.[3] The co-adaptation of the hands, senses, and association areas in precise manipulation seems a first basis for the subsequent development of human intelligence.

4) *Carnivorous-omnivorous diet.* Man and the tarsier are unusual among primates in being carnivores. Many monkeys are omnivores and take small animals as prey. Man is unique among living primates in taking large animals for food and these in large numbers.

Fortunately we have some fossil evidence on the problem of diet. It is still an open question whether the Australopithecines were hunters or the hunted. But by Middle Pleistocene times the *Pithecanthropus* of Peking were hunters of large mammals as well as gathers of hackberries and other plant food.

The change to a partially carnivorous diet had extremely broad implications for the social organization of early hominoids. Carnivores get a large supply of calories at each kill. This concentrated food is more easily transported to a central, continually used shelter than is low-calorie plant food, especially before containers were available.

Whoever killed the baboons and bucks associated with the Australopithecines must have been tool carriers as well as tool users. Tool carrying implies a degree of conceptualization not required in the occasional use of tools (White, 1942; Bartholomew and Birdsell, 1953). Before starting on the hunt there must be a minding which associates the tool with an event which is to occur in the future. This type of mentation has not been observed in captive chimpanzees or monkeys, and certainly not in wild non-human primates. The archaeological record shows it was a consistent part of *Pithcanthropus* behavior by Middle Pleistocene times.

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[3] Probably no one today knows the exact significance of this. See C. Judson Herrick, The Evolution of Human Nature, University of Texas Press, Austin, 1956 for suggestions on the evolution of human mentation.

Compact animal protein high in calories is a good basis for food sharing. Of non-human mammals it is only the carnivores that share gathered food. It is unlikely that the long dependency of human children—so important in the acquisition of culture by individuals—could develop in a society without food sharing. And the amount of information which needs to be transduced in a communication system for plant eaters like the gibbons is small compared to that needed in group-hunting of large animals. Gibbons share, by vocal communication, knowledge about the location of food collected and eaten individually on the site; hominoids share in the location, collection, and consumption of food.

5) *Cortical control of sexual behavior.* There seems little danger that modern anthropologists will overlook the importance of sex in the evolution of culture. Some of us fail to emphasize that, with regard to the physiology of sexual behavior, man is neither a) completely like most other beasts, nor b) completely different from non-human animals. Here, as in many other biological characters, the apes and man are alike and man and the apes are unlike other mammals. In the majority of mammals sexual behavior is seasonal and the sexual periods correspond to times when the female has high probability of ovulation and conception. In such mammals including the lower primates, copulation is evoked by an increase of gonadal hormones in the body fluids. In such animals we can bring about, or prevent, copulation by gonadectomy and hormonal injections. But in man and the chimpanzee, and probably also in others apes, copulation is strongly under cortical control and is not prevented by gonadectomy (Ford and Beach, 1951).

An important adaptation for culture is the change from built-in nervous pathways to neural connections over association areas (where learning and symboling can be involved) in the physiological control of activities like sleep, play, and sex. Cortical rather than gonadal control of female sexual receptivity may not be essential to the hominoid family (observations on other animals suggest not), but cortical dominance in sexual activity may have contributed to the easy transition of the family from a social unit where sex and reproduction were more important than food economy to a unit where subsistence is the dominant familial function.

6) *Vocal communication.* Human speech is an overlaid physiological function. It uses a set of body parts of quite diverse primary action. Consider the muscles used in speaking. Most of our coordinated muscular movement involves corrections and adjustments from proprioceptors. But the laryngeal muscles lack proprioceptors, and feedback control of speech comes by way of the ear and the 8th cranial nerve. When we talk, the voice box, tongue, and lips must work together smoothly and precisely. The 10th nerve controls the adjustment of the vocal cords and the 5th nerve the movement of the lips. Both of these involve branchial muscle while the 12th nerve moves the tongue with somatomotor muscle. The neurological basis of speech is not clear, but it is clear that the only place where the motor organs and steering apparatus of speech are wired together is in the cerebral cortex. Perhaps hand-tool manipulation in group activities like hunting coordinated by vocalization may have helped to make the connections.

Although the larynx is homologous in all primates its position in the throat differs in man. The larynx of quadrapedal primates from the lemur to the chimpanzee is in close to slight contact with the soft palate. This is why chimpanzees cannot make long, resonant sounds. As a consequence of upright posture and flexion of the craniofacial base, the larynx in man is moved down the throat away from contact with the soft palate, and an oral chamber is formed which makes possible resonant human phonation (Kelemen, 1948; von Bonin, 1955).[4]

This is not to deny a rich variety of vocal production to the chimpanzee and other primates. The position of the larynx, however, is one reason why attempts to teach chimpanzees English have failed. Unfortunately no one has tried seriously to teach a chimpanzee to learn to speak using chimpanzee "phonemes."

7) *Expansion of the cerebral cortex.* Current statements in the anthropological literature regarding the size of man's brain often involve misintepretations in one or the other of two directions. On one extreme, some investigators stress the fact that, compared with *mammals* in general, especially large mammals, man's brain is unusually large, both absolutely and relatively. For example, a 150 pound man has a three pound brain, while a 150 pound sheep has a one-quarter pound brain, and a 1500 pound cow has a one pound brain (Brody, 1945). On the other extreme, the stress is put on the conclusion that man's brain is

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[4] This was written before the publication of E. Lloyd DuBrul, Evolution of the Speech Apparatus, Charles C. Thomas Publisher, Springfield, 1958.

indeed large, but not unexpectedly so. For example, when the log of brain weight in *primates* is plotted against the log of body weight, the slope of the regression line is steeper than it is among mammals in general (proportional to the 0.79th power of body weight in primates, the 0.66th power in mammals), and on visual inspection the plot shows—as log transformations often do—remarkably little scatter (see von Bonin, 1952, Fig. 2), suggesting that brain weight in modern man is just about what would be predicted given the general regression of brain on body weight in primates and a knowledge of man's body weight alone (von Bonin, 1945, 1955). But if we take 1345 gm as a brain weight typical for modern man, say of 60 or 70 kg body weight (Bailey and von Bonin, 1951), we find man's brain is significantly larger than the value of 1095 gm of brain for 70 kg of body, predicted by von Bonin's (1955) regression formula: log brain weight = 0.79 log body weight — 1.00. A conclusion which avoids both extremes might stress at least two reasons for man's large brain weight: a) about 80% of man's brain weight may be explained because he is a primate of large body size, and b) about 20% of man's brain weight results from an evolutionary increase in the relative size of hominid brains—resulting in a total brain weight which is vast compared with mammals in general, and is significantly large compared with primates in general.[5] One reason we have overstressed the size of man's brain, even among primates, is that the chimpanzee and gorilla have relatively small brains, especially for primates. Similar arguments suggest that the frontal lobes in man, while well developed, are not of extraordinary and unexpected volume compared with other higher primates.

The distinctive feature of the human cerebral cortex is not so much in overall volume nor in relative size of the frontal lobes, but rather in the way that the projection areas are connected with association areas, especially in the temporal lobes, and in the way the whole thing works. I want only to point to these gross anatomical facts; Doctors Gerard and Washburn will take up their interpretation for cultural behavior.

Rates of human evolution. In closing let me call attention to two sets of observations about rates of human evolution, one from paleontology and one from neontology. The first has to do with the rate of hominoid evolution as measured in genera per million years. Consider some sequence like this one (based on chronological data from Zeuner, 1954):

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[5] In the oral presentation of this topic, I put more emphasis than now seems justified on the thesis that man's brain is only as big as one would predict for a large and typical primate.

PERIOD	MILLION YEARS AGO	GENUS
Miocene	12 (25)	*Proconsul*
Lower Pleistocene	1	*Australopithecus*
Middle Pleistocene	0.5	*Pithecanthropus*
Upper Pleistocene	0.25	*Homo*

Using only known forms, without guesses about unknown ones, this sequence of 4 genera is close to a minimum one for hominoid evolution. Almost everyone would agree that these are good genera, although the taxonomic distance between *Pithecanthropus* and *Homo* is not so great as between the others. If one insists that additional genera must be put in (and probably there ought to be at least one more for the Pliocene) it will only strengthen the conclusion I want to draw. The point is that there has been an unusually rapid rate of hominoid evolution as measured in genera during the past 12 million years, and especially in the past million. This is apparent when we compare hominoid rates with those for horses, chalicotheres, and ammonites (table 1). Something has speeded up hominoid evolution. I would guess that selection (perhaps within-species or inter-group selection) for a new type of environment —a cultural environment—has a lot to do with it.[6]

TABLE 1

Rates of evolution in terms of genera per million years
(Data, except hominoids, from Simpson, 1953)

LINE	NUMBER OF GENERA	MILLIONS OF YEARS	GENERA PER MILLION YEARS *
Ammonites	8	160	0.05
Horses	8	60	0.13
Chalicotheres	5	38	0.13
Hominoids	4	12	0.25
Hominids	3	1	2.00

* For extant lines: Number of genera minus one per million years.

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[6] C. H. Waddington has suggested a non-Lamarckian mechanism whereby variations in ontogenetic pattern initially brought about by environmental influence may, if subjected to strong selection, undergo genetic assimilation. See his The Strategy of the Genes, George Allen and Unwin Ltd., London, 1957.

Ontogenetic rates. Insofar as phylogenetic information can be deduced from observations on living animals, there exists among primates a general evolutionary trend to increase the duration of the main periods of the life cycle. The evidence is presented in table 2. In the great apes the gestation period is lengthened to at least 34 weeks, full

TABLE 2

Average duration of prenatal and postnatal growth periods and of life span in different primates (Schultz, 1956)

PRIMATE SPECIES	GESTATION (WEEKS)	MENARCHE (YEARS)	ERUPTION OF FIRST AND LAST PERMANENT TEETH (YEARS)	COMPLETION OF GENERAL GROWTH (YEARS)	LIFE SPAN (YEARS)
Lemur	18	?	?	3	14
Macaque	24	2	1.8–6.4	7	24
Gibbon	30	8.5	? –8.5	9	30
Orang-utan	39	?	3.0–9.8	11	30
Chimpanzee	34	8.8	2.9–10.2	11	35
Gorilla	?	9	3.0–10.5	11	?
Man	38	13.7	6.2–20.5	20	75

growth is attained by the end of the 11th year, and animals in their 3rd decade are senile. In man the duration of the prenatal period has changed little, if any, from that characteristic of the great apes, but the duration of the period of postnatal growth has almost doubled, and the total life span has more than doubled. Man is not unique with regard to the gestation period, but he is specialized in the marked elongation of postnatal growth and the long postponement of the onset of senility. These human specializations are extremes of trends found to lesser degrees in the evolutionary history of other primates (Schultz, 1956).

Man, then, is not much different from other primates, especially the apes, in the general sequence of events from conception to birth. After birth, the ontogenetic pattern in man differs markedly from that of all non-human primates but differs in a direction forecast by the general trend of primate evolution. I would guess that this elongation of the life periods after birth is a consequence of physiological adaptation to the acquisition of culture. Culture is a biological adaptation with a non-genetic mode of inheritance depending on symbolic contact rather than fusion of gametes. It has greatly supplemented somatic evolution. In all known human societies, individuals participate in social systems whose members represent more than a single biological family in which all are connected (as the social insects are) by gametes from one parental set. No human family is a self-sufficient system of social action. Symbols rather than gametes make this so. It may be assumed that the genes controlling the growth cycle in man have been changed through selection to man's *human, cultural* environment.

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BRAINS AND BEHAVIOR

BY RALPH W. GERARD
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SINCE I am not an anthropologist but a biologist, a physiologist primarily, I take it that I am to discuss not culture but the physical, the biological, substrate of behavior. This is a pleasant assignment because living things really matter only in terms of their behavior and how they modify the environment. The most primitive organisms and the most advanced are pretty much alike in their dynamic equilibrium, the intake, digestion, metabolism, and excretion of food. The details differ but the same substances pass essentially through the same steps. The processes of growth and reproduction also are vastly different in detail but still basically alike in the chromosome and gene machinery.

It is primarily in ability to interact with the environment that the big changes have come in evolution and, of course, man is the outstanding interactor. Man has succeeded enormously in that there is probably more living protoplasm of the human variety than of any other protoplasm; certainly man has modified the environment more than all other organisms combined and is the greatest catalyst that nature has produced so far. This is because of his behavioral complexities, his repertoire of interactions, particularly his ability to interact with his fellows and collectively to do things that an individual man could not begin to do. I should like first to consider with you the evolution of behavior, and primarily in terms of the mechanisms that make it possible.

Some kind of behavior is present in every organism; it must in some way be related to its environment. This may in effect be nothing more than chemical interchange, although every cell has some ability to rehabilitate itself, some responsiveness and, to some degree, can conduct and transmit. But this is at the cellular level, and perhaps the major difference between plants and animals is that in plants these primitive behavioral capacities have remained overwhelmingly at the cellular level. Animals introduced some new tissues, new structures, and began to specialize in sensation, in conduction, in response. This is the great step that animals made. Even this didn't really become exciting until animals stopped being sessile and were able to move around in their environment. Not just to move a little in a radially symmetrical manner, in which any direction is as good as any other; this was not too different from no movement.

The big change came when animals developed a direction, a head end, a longitudinal gradient, and bilateral symmetry. One end was then in front, faced the future, met the environment, got the bumps. The nervous system showed a progressive movement towards that end, with a centralization of the cells and neurons scattered throughout the organism. Then came the development of distance receptors, new kinds of sense organs that gave greater information about the environment the organism would encounter ahead as it continued crawling, rather than what was happening to its tender skin after it had already arrived. Along with movement came also a premium on speed and control, with comparable evolution of the conduction mechanisms and the nervous system itself.

Now, this great invention and development of the units culminated, at least along one line, with the origin of vertebrates. Earlier there was great improvement in receptors, conductors, and detectors. The human eye is sensitive to 10^{-8} candle power; the clam eye only to 10^{5}. That is a ratio of 10^{13} times in sensitivity, 10 million million times greater sensitivity to light in our eye than in the clam's. The human muscle gives its twitch in a few hundredths of a second; the sea cucumber muscle takes three hours—a factor of a million. The medullated vertebrate nerve can conduct at over a hundred meters a second; the primitive jellyfish nerve conducts a centimeter in a second or longer. Again the factor is a hundred thousand or that order. Another very interesting point is that, with directional behavior and higher organization, came irreciprocal conduction in the nervous system. In primitive nervous systems, messages can go in either direction across the junctions between separate neurones, for the synapses are symmetrical structurally and functionally. Later, synapses become asymmetrical and permit only unidirectional passage of messages and information through the nervous system. I would again like to emphasize, however, that all this was essentially completed with the arrival of the vertebrates, not of man, or of the primates, or even of the mammals. The hawk's eye, in some respects at least, is better than man's; the dog's smell is better than man's, the snake has a temperature receptor that only our latest snooperscopes approach for infra-red and temperature measurements, of .005° C, and so on down the line.

What happened after the units were developed? Further advance had to be in improved connections and interactions and in sheer number of units. Let me exemplify this with the development of radios. Can you remember back to the early triode? When the heterodyne and superheterodyne circuits came in, the number of tubes in the radio multiplied—a good set was one with a dozen or two. With improved tubes, the tetrode, pentode, and now the transistor which functions on a different basis, the unit itself became much more efficient and good radio sets now need only half a dozen tubes. Moreover, these could be miniaturized, the circuits printed, and the whole instrument represents our present technological level of advance. In the case of the nervous system and animal behavior, the unit in frog or fish is essentially as good as that in man; the frog's nerve conducts as fast as man's at the same temperature, and so on down the line. Improvement from fish to philosopher, then, must depend either on better circuits, better interconnections between neurones, or simply on more neurons.

The circuits are certainly important and, although we are far from understanding the full detail of how they operate, we have a good general picture. In contrast, little parallel attention has been given to sheer number. In playing Twenty Questions, if the number of questions permitted is limited to, say, two instead of 20, or increased to 200, the elaborateness or complexity, the intellectual richness of the material which can be handled, shrinks or expands dramatically. The modern computing machines have essentially the same units, whether simple little ones or the great "—IACs"; the differences are in more and more units, which increase performance not merely in a summative fashion but in some kind of multiplicative one. It is rather like Eddington's lovely statment, to the effect that we once thought if we knew "one" we knew "two," for one and one are two; but we have since found we must learn a deal more about "and." A large organization composed of the same kinds of units, say human beings, develops differentiation of labor, organizational patterns, and is able to do a great many things that a smaller one cannot, simply as a matter of numbers. Well, as long as behavior flowers while the units remain essentially alike (despite marked differences between neurons in different parts of our own nervous system and, to some extent, between those in different species, there is no progressive improvement in them); there must be some relation between total number of neurons, size of nervous system, and behavioral capacity.

I am interested in the statment of Spuhler (chapter 1, above) that extrapolation in the primate series indicates that man's brain is bigger largely to the degree expected because he is bigger. Maybe this is all there is to it, and little spurt of special cerebral evolution has occurred; but then the simple size increase was fortunate, for certainly man's big brain is what makes possible man's human behavior. This can be pointed up in some detail. I remind you that the size of the motor area which controls various muscles does not relate to the size of the muscle but to the skill in using it. I strongly suspect that you could not teach a chimpanzee to speak chimpanese, let alone English, because he doesn't have large enough motor areas for his tongue and his larynx. The motor area for the human tongue is much larger than that for his whole leg. The human area for a tail is non-existent; but in the spider monkey, with a wonderfully prehensile and manipulatable tail, the tail area is huge.

Let me take the remaining time for a little more concrete guessing—for it is really that—as to mechanisms in the nervous system. I shall consider first some structural and then some functional ones. With evolution, with cumulative racial experience starting in the individual and getting fixed in the race, by the genetic mechanisms mentioned, certain basic patterns are laid down. Thus, the reflex arc is standard equipment. Later came mechanisms higher up the neuraxis: first those related to awareness in the mid-brain; then those serving emotion, pain in the thalamus or between brain, and pleasure still higher in the old forebrain; and finally reason, with its attributes on which we pride ourselves, in the new forebrain or cerebrum. Further, two aspects of consciousness probably depend on two kinds of mechanisms. The content of awareness is probably related to the specific paths from the thalamus to the cortex, which carry localized patterns of activity that are distinctive for the individual past experience of that particular organism, and the current pattern of sensory stimulation. The general level of consciousness, rather than its content—whether one is drowsy or alert or excited—however, depends on a non-specific activating system from thalamus to cortex and partly, I believe, on the adrenalin level in the blood, which increases as part of the general emergency response of an organism to a threatening situation.

Next, I must point out that a major decision was made by animals quite early in evolution when they adopted, so to speak, an alphabetic rather than an ideographic language. The nervous system is made up of relatively uniform units, which can be combined in various ways, rather than of highly particularized units, each one of which stands for something specific. The proof of this is the existence of synapses. The synapse can only be present because it is important *not* to have a message go through on an express track from one receptor to one effector. Otherwise, why break the nerve path and cause slowing, the chance of confusion, and all the rest? The synapse permits changeability, allows the units to connect now this way, now that way. This leads to the last point on structural aspects. I prefer to think not of afferent and efferent flows involving the nervous system, but of information coming in and instructions coming out. Every synapse is thus, in effect, a decision point. A message comes to it from the pre-synaptic fiber; does it go out over the post-synaptic one or doesn't it go out? That is the decision the nervous system makes—at a near infinity of places and times. As to how this decision is made, I will have a word in a moment.

The great organizational problem of the nervous system I suspect, is what we might call, in military terms, its "table of organization." To what extent, for example, are these decision-points scattered peripherally, so that there is effective local reflex control of parts of the system, and to what extent are they centralized, so that there is a general or a president whose word is relayed down through subordinate officers and out to the executive agents, the privates or workers. The degree of true centralization, I think, is going to prove the most interesting morphological and morphophysiological question to ask about the nervous system. It hasn't been asked; I suggest that it is very much worth asking.

Let us, finally, take a quick look at some of the physiological and functional aspects of neural mechanisms. Much attention has been paid to morphology, especially in relation to neural evolution, because records are morphological and because, in the development of any science, the morphological phase precedes the functional one. There have been few efforts to look at the functioning of the nervous system not in terms of where does it happen, which part does this or that; but in terms of what differences in physiological properties—either of the unit neurons or of the patterns of their synaptic connections—could be responsible for this or that performance. This is, of course, much too large, and perhaps esoteric, to present in the few minutes left, but some important items follow. First there is growing evidence (but no solid proof) that the nervous system works in a series of time frames, perhaps of a tenth of a second. Next, there is evidence that impulses reverberate—run many circles around a neuron loop—and that this may be involved in the process of fixing experience. Unless there is time for a message to reverberate many thousand times, a memory is transient and experience does not leave a permanent trace. Third, attention is related to threshold changes in primary receptors. A mother can hear her baby's cry through general noise because of a positive feedback to the ear itself. When an animal is conditioned to listen for a particular sound, the potential evoked by it in the ear is increased; if conditioned to ignore it, the reverse occurs. Fourth, are clues as to the processes of reason and imagination and of the introduction of the novel.

Let me now put a few of these things together. I warn you that this is pure guesswork but I believe it is at least an educated guess. Suppose differences between nervous systems were examined in terms of one or another of the properties that make for transmission across synapses, or for reverberation, or for synchrony, or for laying down memory traces. If memory traces are fixed more easily than the norm in some individual he might have a quiz kid kind of mentality. Everything that comes in would fix pretty easily and a tremendous amount of memory residue accumulate; but, by the same token, if things fix too easily there should be less flexibility. I think it is the case that people with phenomenal memories are rarely very imaginative or creative. With poor fixation, on the other side, would come presbyphrenia, failure of recent memory in the old person, and the unpleasant ability to go through complicated interactions and, at the end, forget all about them.

Look next at threshold fluctuations. It is established that the threshold for a given synapse is not constant, aside from actual manipulation, but shows erratic, unpredictable spontaneous changes. These depend on random events—which means, on such submeasurable and unanalyzed factors as electron movement, Brownian movement, and the like. Now, a certain amount of this is very fine, because if there were none every input would find the system (once the basic paths were established) exactly the same so its output would be automatic and routine and there could be no innovation. This is the situation for spinal reflexes and emotional reflexes, they are pretty stereotyped. But with a play of thresholds, the same stimulus may at one time send impulses along this path and at another time along that path, and so favor innovation and imagination. If fluctuation goes too far, coherence would be lessened and flight of ideas occur. Finally consider the phenomenon of synchronization. Neurons are known to have electrical beats, and large numbers of them beat in phase, as armies marching in step. When impulses come in, from the eyes particularly, the neurones begin to scintillate, to get out of step with each other, and the brain wave rhythm breaks up. Suppose there are differences in the firmness of synchronization. Too firm a synchronization should give what I might call "tubular attention"—a person with blinders on all sides who follows one path and cannot be diverted to see anything else. With too weak synchronization, neurons would get out of step too easily, and flightiness and inability to maintain attention would result. With excessive inhibitory action there should be decreased attention or actual depression; with too little inhibition there could be anxiety and even mania or epilepsy. These are some physiological variables that I am suggesting as important for behavior—another problem for the future.

What I have undertaken is just to point up ways in which modern neurophysiology (or neurobiology) can begin to understand the nervous system as the substratum of man's capacities, as an organ which can play the tunes of behavior. For the future, with the recently added tools for increasingly precise manipulation of the brain and measurement of behavior, progress should be rapid. Fine electrodes can now be left in place indefinitely, and the brain activity sampled or excited. Animals can administer shocks to their own brains, or inject them with drugs. More specific, "psychoactive," drugs are now available as biochemical instruments to manipulate enzymes and so alter these physiological properties of neurons—to say nothing of their therapeutic value for the mentally ill. Experimental evidence on the kinds of relations between neural mechanisms and behavioral patterns may thus soon be at hand and lead to sharper guesses than those I have put before you.

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SPECULATIONS ON THE INTERRELATIONS OF THE HISTORY OF TOOLS AND BIOLOGICAL EVOLUTION

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THE purpose of this symposium is to consider the interrelations of the human animal and his culture. The point I would like to make in this brief paper is that much of what we think of as human evolved long after the use of tools. It is probably more correct to think of much of our structure as the result of culture than it is to think of men anatomically like ourselves slowly discovering culture. The way of life of early man (pre *Homo sapiens*) must have been very different from that of our immediate ancestors. It is not certain what limits their biology may have placed on their way of life, and therefore, Hallowell (1956) has suggested that it is better to speak of the early men as possessing "proto-culture," and keeping the word "culture" for the way of life of *Homo sapiens*. As Hallowell points out, when we are dealing with anatomically primitive men, it is uncertain whether their intelligence, capacity for language, art and social organization were like ours. Probably, the biological potential for culture of Wadjak man was the same as our own, but the biology of Pekin man placed quite different limits on his proto-culture.

If we want to relate the evolution of culture to biological evolution, a first requirement would be fairly complete records, which do not exist. The biological record is extraordinarily scanty, as Dr. Spuhler (chap. 1, this vol.) has already indicated. The archeological record is richer but it is difficult to interpret and there are great disagreements. One does not find culture or proto-culture, but only clues as to what the way of life may have been. I am going to indicate a possible interpretation of the sequence of tools, and then speculate on the relations of this sequence to the fossil men (fig. 1).

It is possible that pebble tools are the only tools surely present before the time of the second glacial advance. These simple forms may have been invented many times, and they are too crude to give indications of cultural contact. Subsequent to the pebble tools appear traditions of

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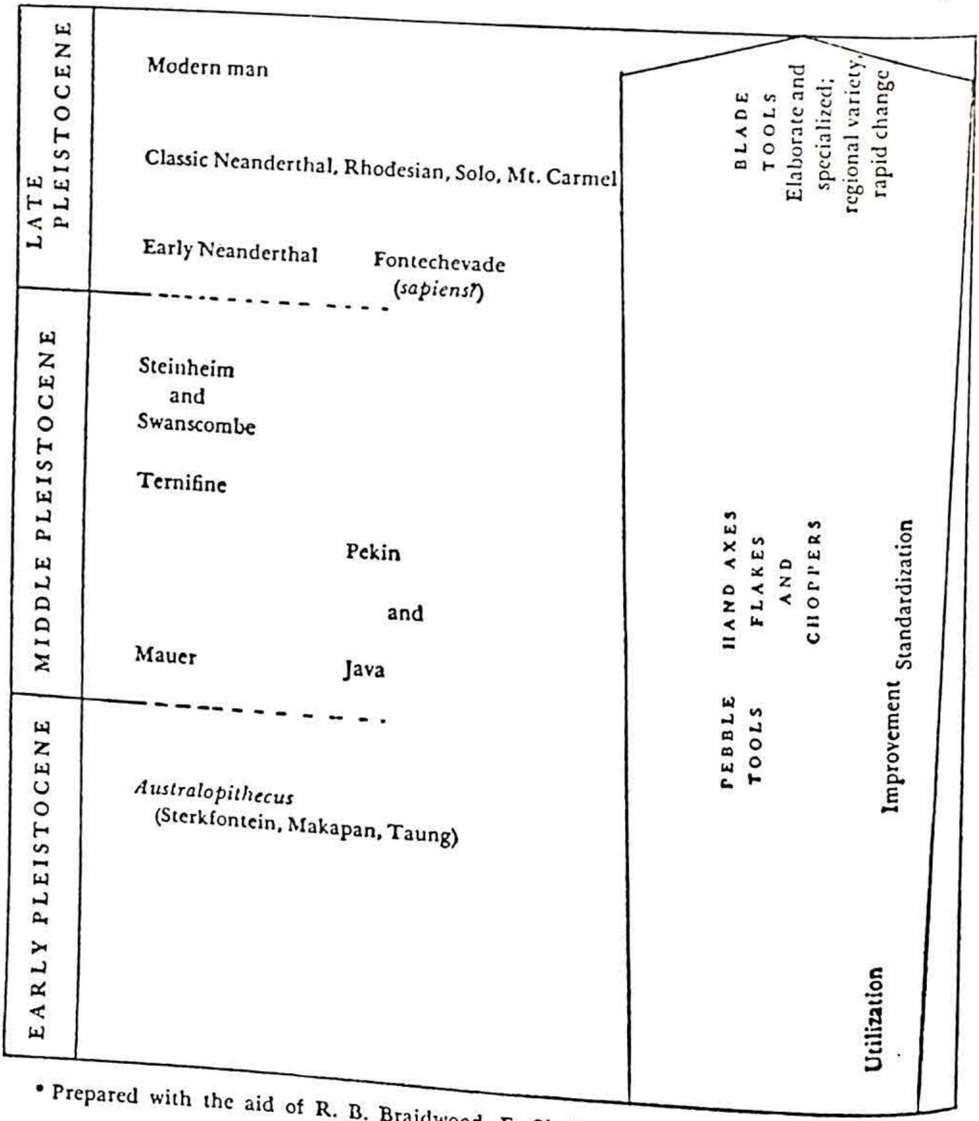
manufacture, tools which were made according to such complicated plans that one feels that there must have been communications among the makers. The great biface and chopper traditions extend over huge



* Prepared with the aid of R. B. Braidwood, F. Clark Howell, and Kenneth P. Oakley.

FIG. 1. A POSSIBLE CORRELATION OF TYPES OF TOOLS AND FOSSIL MEN.

areas of the Old World, from second glacial times on into the last interglacial period. The persistence of these traditions is something which seems very strange to us today, for in subsequent layers the rate of change is vastly greater. Probably, the slow evolution of these proto-cultures (which we know only from the fragmentary record), is due in part to the bilological limitations of the early men. The rapid rate of change which appears during the time of last glacial advance is due both to the cumulative effects of culture and to the appearance of *Homo sapiens*, the biological form which made culture as we know it possible. As soon as *Homo sapiens* is surely on the scene, there is great regional differentiation. No longer does an Acheulian tool extend from Africa to India to Europe, and last for 100,000 years. Tool types change rapidly. Man crosses large bodies of water, conquers the Arctic, and art appears in the archeological record. This is the record of culture, the evidence of the presence of the restless creator we know as *Homo sapiens*.

According to this brief outline, there are three biological and cultural stages in the Pleistocene: an early-Pleistocene—Australopithecine—pebble-tool stage; a middle-Pleistocene—early man—biface, chopper stage; and an upper-Pleistocene—*Homo sapiens*—Upper Paleolithic phase. For the rest of this talk I want to speculate about this view of human evolution, to consider its implicaton, and to think about what kinds of evidence might help to prove, or disprove, it. As Spuhler (chapter 1, above) has indicated, man became distinct from the apes when he became a biped. The structure which made this change in locomotion possible is primarily in the pelvis and associated muscles, and it is because the innominate bones of four different specimens of *Australopithecus* all show a condition far closer to man than ape that these forms are regarded as bipeds. Since these small-brained bipeds of the early Pleistocene may have been the first tool users, I should like to be able to say something about their hands. Unfortunately none are preserved and as we all know, a whole hand is unlikely to be. However, it is not necessary to find a complete hand to make some deductions about its use. The finger bones are very different in the quadrapedal monkeys and in the arm-swinging apes. In the apes the phalanges are curved and the edges are highly developed. With a single bone it might be possible to give a highly-informed guess as to whether the australopithecines still used their hands for climbing frequently. It is not unreasonable to hope that such small fragments may be found. Further, man has much more motion in the wrist between the radius and the first row of carpal bones than monkeys do. The degree of motion is accurately reflected in the joint surfaces, and can be estimated from quite small fragments of bone. A Miocene ape in which the hand is well preserved, probably had motions of the hand and wrist much more like a monkey than like man. I suspect that a late Pliocene brachiating ape was the ancestor of the Pleistocene australopithecines, and that the structure of the hand which made tool use possible was evolved at that time.

It has been argued that ape hands are too specialized and different from those of man for a human hand to have evolved from the sort of hand seen in the living apes. Obviously, if our hand evolved from an ape hand, it was from a form long dead, and the hand may have been somewhat different from those of the living apes, but the essence of this argument is the assumption that the hand which first started to use tools was much like that of living men. However, if an early australopithecine with an ape-like hand had started to use tools, selection would have changed its descendants. Once these animals became bipedal ground dwellers, using tools a little, then selection would favor shorter fingers and larger thumbs. The use of tools would change the direction of evolution and the form of the hands. The essence of the interpretation of evolution by selection is that selection causes change, and I believe that the form of the human hand *is the result* of the new selective pressures which came in with the use of tools. According to this theory, an ape's hand was pre-adapted to the use of tools. The hand was freed by the assumption of bipedal locomotion. Then new selection pressures coming with the use of tools changed the ape hand into the human hand. Our hand is the result of at least half a million years of tool use. The uniqueness of the human hand, those features which distinguish it from the hands of apes, is the result of culture. According to this theory, it is futile to look for an ape-like ancestor with a large, fully opposable thumb, because the human thumb as it exists today evolved after bipedal locomotion and with the use of tools.

Turning from the hand to a more general view of human evolution, many people used to believe that men essentially like ourselves lived at the beginning of the Pleistocene. A common form of phylogenetic tree shows *Homo sapiens* as already in existence before the earliest tools. In such a tree all the anatomically primitive fossil men are put on numerous side branches. This led to attempts to relate the *sapiens* fossils to the biface tools and the anatomically primitive men to the flake tools. This type of correlation no longer is tenable because the supposedly early *sapiens* fossils have been discredited one by one. The separation of the flake and biface traditions was a great oversimplification. Undoubted associations of primitive forms of man and the Acheulian tools have been made. As Spuhler has indicated (chap. 1), it is now extremely unlikely that all the primitive forms belong on side branches. The general view of evolution outlined by Weidenreich (1947) is probably correct. According to Weidenreich, the men of the early Pleistocene were primitive in form, and *sapiens* evolved late.[1]

A possible evolutionary sequence is, the australopithecines of the early Pleistocene, Peking man and other primitive forms of the middle Pleistocene, and *sapiens* of the end of the Pleistocene. There is a clear difference between australopithecine and ape in the dentition, but most of the evolution of the brain comes after the australopithecine stage. It is my belief that the decrease in the size of the anterior teeth and the tripling of the size of the brain came after man was a tool user, and were the result of the new selection pressures coming in with the use of tools.

At Sterkfontein, Robinson and Brain have now found pebble tools. The implements and conditions of discovery have been examined by Desmond Clark, and there is no doubt that these are tools coming from the australopithecine levels and made of stone which does not naturally occur in the deposit. The evidence has been summarized by Oakley (1957) and he believes that the tools were made by *australopithecus*. I think that this is the most probable interpretation, but some think that no creature with so small a brain could have made tools and that more advanced forms of man must have made them, perhaps using them to kill the australopithecines. This sounds like the history of Peking man all over again. However, one cannot be sure, and, as pointed out to me by Dr. Robert Braidwood, this sort of controversy can only be settled by further careful excavation. An association in one site cannot settle the matter, and the situation is further complicated because several kinds of *Australopithecus* and *Homo* may have made tools. It is perfectly possible that different species of australopithecines and early *Homo* lived at the same time and *all* made tools. However, at the present time no representatives of *Homo* or bifaced tools have been found in the australopithecine sites. The use to which these earliest tools were put is uncertain. What is needed in archeology is more information on their possible use, as this would be of the greatest value in interpreting the adaptation of the animals. At the moment the best guess seems to be that the australopithecines were tool-using gatherers and scavengers.

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[1] This view of evolution was usual in Germany and eastern Europe, while the early *sapiens* theory was characteristic of France and England. In general, the nations which accepted Piltdown also believed in very early *sapiens*. In this country, Hooton's views were typically English and Hrdlička's as typical of the area where he was educated. The two theories, supported by strong national biases, originated long before the discovery of the best documented and most important fossils, and before modern archeology. It is necessary to re-examine the primary evidence. At present there is no single fossil of *Homo sapiens* in which a substantial part of the face and of the braincase is preserved which is dated before the last glaciation.

By around 900 cubic centimeters of cranial capacity, men were making well-shaped, finely formed tools. These men of the middle Pleistocene were certainly hunters, and Spuhler has already mentioned the importance of the hunting habit. Our ancestors can have hunted large animals only with the aid of tools, and the first certain evidence of this comes with the biface or chopper traditions. Even with tools, hunting must have involved cooperation within the group. The use of tools must have led to psychological changes. Compared with their primarily vegetarian ancestors, the men of the Middle Pleistocene must have been more aggressive outside the group and more cooperative inside. Sharing food within the group, cooperating in the hunt, sharing traditions of tool making; these are the positive sides of the changes which came in with large scale hunting. These contrast to the fearsomeness of the hunter to other kinds of animals and lead to the possibility of war. Man has a carnivorous psychology. It is easy to teach people to kill, and it is hard to develop customs which avoid killing. Many human beings enjoy seeing other human beings suffer, or enjoy the killing of animals. After all, public spectacles of beatings and tortures are common in many cultures. This is the characteristic behavior of carnivores and is not seen in apes and monkeys. Much of what we think of as normal human nature may date back only to the hunting traditions of the Middle Pleistocene, while bipedalism and tool use are of much greater antiquity.

TABLE 1

Representative cranial capacities

1200–1500 cc	*Homo sapiens*
900–1100 cc	Java-Peking
450–550 cc	Australopithecines
350–450 cc	Ape (primarily chimpanzee)

A slightly more detailed view of the evolution of cranial capacity is given in table 1. Dr. John M. Roberts, of the University of Nebraska, and I worked on this last year at the Center for Advanced Study in the Behavioral Sciences. This analysis is the result of our numerous discussions at the Center. The first problem in considering the evolution of the brain is to get estimates of size. Since the fossils are not numerous, many are fragmentary, and estimates in the literature vary greatly, we have therefore resorted to a device which we call "representative capacities." These are the capacities which we think best represent a group of fossils. All the most frequently occurring values lie in the range given. All the figures mean is that 450–550 is about as good an estimate of brain size in the australopithecines as 1200–1500 is for the men of the Upper Paleolithic. We have tried to keep body weight approximately the same. For this reason ape capacities are estimated from chimpanzee primarily, rather than including gorilla with three times the body weight. An *Australopithecus*, one might guess from the bones from Swartkrans, might have weighed around 100–140 pounds, and such an animal would not differ greatly in size from chimpanzee or man. We think that there is a real increase in brain size of approximately three time, at comparable body weight, insofar as that can be estimated in the exceedingly fragmentary fossils. Pebble tools go with the australopithecine-size brain; traditions with the Java-Peking primitive-man size brain; and figures for early *sapiens* do not differ from those for Neanderthal. Capacity is a very crude measure of brain evolution and there may well have been numerous other changes which do not show in the fossils. However, increasing the number of cells is very important, as Gerard mentions in chapter 2. Size does reflect increasing number of cells, but due to folding of the cortex it may lead to an underestimate.

In the expansion from an ape-type brain to a human type, the different parts of the brain did not expand equally. Figure 2 is a diagram from Penfield and Rasmussen (1950) showing the way the human body is represented on the cortex. It will be seen in the diagram that the body is very unequally represented in the motor cortex. The areas which are largest are the ones of greatest functional importance. This is true of the mammalian cortex in general. For example, the areas associated with the face are always large. But if the monkey cortex is compared with that of man, it will be found that in the former the area of cortex associated with the foot is approximately as large as that for the hand. When the brain increased in size, the area for hand increased vastly more than that for foot (which remained stable or, perhaps, decreased slightly). This supports the idea that the increase in the size of the brain occurred after the use of tools, and that selection for more skillful tool-using resulted in changes in the proportions of the hand and of the parts of the brain controlling the hand (including primary motor and sense areas, and the areas concerned with the elaboration of skills).

Again, following Penfield and Rasmussen, the areas of the cortex concerned with speech are very large. The reason that a chimpanzee cannot learn to talk is simply that the large amounts of brain necessary for speech are not there. The frontal lobes of man are greatly expanded also, and these areas are, at least in part, concerned with elaboration of thought and planning. Foresight and planning are essential to any complicated social life, and in the future it may be possible to demonstrate that the expansion of much of the cortex is directly related to new selection pressures associated with the evolution of complex social systems. Our brains, then, are not just enlarged, but the increase in

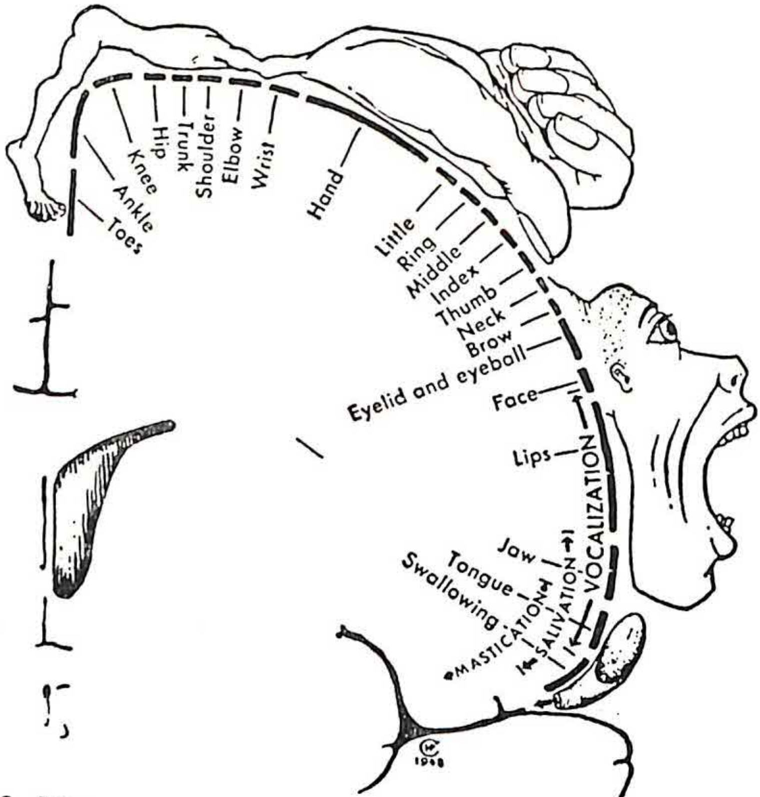

Fig. 2. Diagramatic Representation of the Proportions of the Motor Cortex. From Penfield and Rasmussen, *The Cerebral Cortex of Man*, 1950, copied with the permission of the Macmillan Company.

size is directly related to tool use, speech, and to increased memory and planning. The general pattern of the human brain is very similar to that of ape or monkey. Its uniqueness lies in its large size and in the particular areas which are enlarged. From the immediate point of view, this human brain makes culture possible. But from the long-term evolutionary point of view, it is culture which creates the human brain. Dr. John M. Roberts is investigating the cultural implications of this interpretation of evolution.

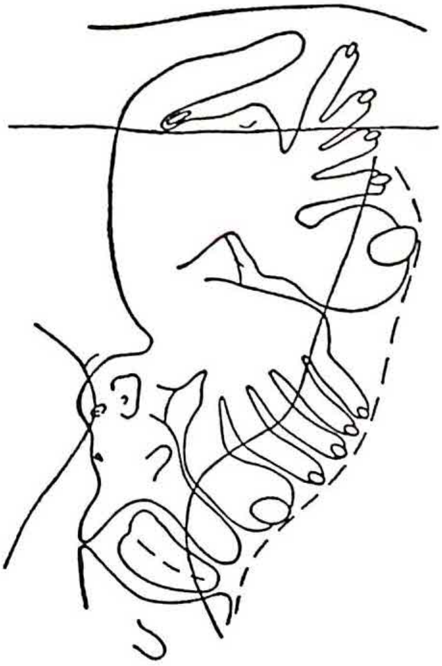

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FIG. 3. MOTOR HOMUNCULUS OF MONKEY. After Woolsey and Settlage. Reproduced from *Handbook of Experimental Psychology*, S. S. Stevens, ed., 1951 by permission of the publisher, John Wiley and Sons, Inc.

Obviously, these speculations range far beyond what can be proved at the present time. But I think that it will be possible to prove much more in the future. In closing, I would like to speculate about these future possibilities. As stated before, a possible evolutionary order is an ape, a tool-using australopithecine, a hunting-speaking complicated tool-making ancient man, and modern man. If we think of such an order, a better understanding of changing diet is of paramount importance in interpreting the fossil record. Spuhler (chap. 1) has already mentioned the lengthening time which the eruption of the teeth take in evolution. The teeth form over a considerable period of time before they erupt, and during this time of formation the developing tooth is affected by its environment. If a growing person is subject to severe starvation or disease, there are characteristic defects built into the structure of the tooth which is being formed at that time. Since the dentition develops over a period of several years, a record of health of most of an animal's youth is frozen into it. The late Dr. E. L. Schuman was carrying out preliminary work along this line, but nobody has developed the necessary methods and systematically applied them. With microscopic x-ray and the tremendous advances in mico-chemical techniques, which Dr. Frederick Thieme is adapting to anthropological uses, major progress seems possible. It seems probable to me that there are records in the teeth and bone which will help us to decide whether animals were primarily hunters or were largely vegetarian. It should be remembered that the teeth of the associated animals can be treated in the same way and may prove equally valuable in reconstructing the ecological conditions.

In the reduction of the dentition, the canines and incisors seem to have decreased in size first. This decrease may have been the consequence of the first use of tools, perhaps of wooden tools which are long since gone. This reduction affected the males primarily, for in female apes the canines are not large. The cause of this sex difference is lost before the australopithecine stage, and it may well be that clubs had already replaced canine teeth in fighting. But among the australopithecines the molar teeth were extremely large. As far as one can tell from the skulls, the temporal muscles were very large too. There is no indication that the chewing part of the dentition started to decrease until after the major tool traditions. This suggests that the preparation of food began in the middle Pleistocene along with hunting of large animals and the biface and chopper tool traditions. Before that time, selection still favored the maintenance of a very large grinding apparatus.

With the reduction of the teeth, (first the canines, then the molars) the face decreased in size. The browridges decreased along with the face, but we have been experimenting on browridges and all the variation cannot be accounted for on the basis of the mechanics of the face. Muscle size and face size account for part of the browridge size, but not for all of it. Browridges are decreased in all domestic mammals.

Pigs, horses, cattle, cats,—all these unrelated forms show characteristic facial changes and no one bred selectively for these features.

The same is true in rats. Tame rats have smaller browridges and faces than wild rats. But here the cause is known. The wild rats have larger adrenal glands, are more prone to rage, and are much more difficult to handle. In choosing animals which are easier to tame, a glandular pattern has been selected which creates differences in the skull. A similar process may have gone on among ancient men, and the most irascible individuals, those who could not fit into the evolving social order, may have become eliminated. This social selection would have affected the form of the human skull.

In summary, it was bipedalism which started man on his separate evolutionary career. But tool use was nearly as early. Biological changes in the hand, brain, and face follow the use of tools, and are due to the new selection pressures which tools created. Tools changed the whole pattern of life bringing in hunting, cooperation, and the necessity for communication and language. Memory, foresight and originality were favored as never before, and the complex social system made possible by tools could only be realized by domesticated individuals. In a very real sense, tools created *Homo sapiens.*

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ANIMAL "LANGUAGES" AND HUMAN LANGUAGE[1]

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FROM time to time, anthropologists draw up lists of the kinds of behavior common to all human beings but not shared with other species. One item found on every such list, though under various labels, is the power of speech.

It is true that human language is a remarkably powerful institution. Yet Man is by no means the only animal that carries on communication. Until we can describe in detail just how human language differs from any variety of communicative behavior manifested by non-human or pre-human species, we cannot really know how much or little it means to assert this particular human uniqueness.

So far as I can see, the only way to find out what we want to know in this area is as follows. First, we must itemize those design-features of human language which seem to be of crucial importance in making it possible for language to do what it does. Second, we must examine all the non-human systems of communication that can be found, to see to what extent each crucial design-feature of human language recurs. It goes almost without saying that in carrying out this program we must not allow ourselves to neglect any point because of its apparent triviality. Indeed, the most important items will surely tend to be hidden from our view because they are right under our noses.

My own research on this problem has so far yielded a list of seven properties of human language that seem to merit consideration. None of these is my own discovery; my role has been merely that of bringing the seven together because of their possible relevance for the particular problem, and, in some cases, of rephrasing a point in such a way as to render it easier to handle.

The first of the seven may be called *duality of patterning*. For some reason, this seems to be the most difficult to explain. I shall therefore begin with a simple nonlinguistic example.[2] Suppose that Paul Revere and his colleague had needed a total repertory of several hundred messages, instead of just two. It would have been inconvenient to have had several hundred lanterns on hand in the Church tower. But it could have been agreed that each message would take the form of a row of 5 lights, each one either red, or yellow, or blue. Then only 15 lanterns would have been needed—one of each color for each position—but the system would have provided for a total of $3^5 = 243$ different messages. We assume that meanings would have been assigned only to the whole messages, so that, for example, "red light in first position" would not have had any separate meaning of its own, but would merely have served to distinguish certain messages from others. This modified Paul Revere system would then show what we mean by duality of patterning: a set of conventions in terms of *smallest meaningful elements*—here the whole messages—and also a set of conventions in terms of *minimum meaningless but differentiating ingredients*—the three colors and 5 positions.

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[1] This is an interim report on a project begun during 1955-6 at the Center for Advanced Study in the Behavioral Sciences. Research continues as time allows, and a fuller discussion will eventually appear.

Although the practice is virtually unprecedented with works smaller than whole books, I should like to dedicate this article to Alfred L. Kroeber, in appreciation of his stimulation and encouragement.

Any human language has this same design-feature. In English, for example, the phoneme /b/ at the beginning of an utterance has no meaning of its own, but merely serves to distinguish *beat* from *meat*, *bat* from *rat*, and so on. Phonemes are the minimum meaningless but differentiating ingredients in a language; the smallest meaningful elements are what we call morphemes.

The principle of duality is a great convenience in any communicative system where the repertory of messages must be large, since a relatively small stock of minimum meaningless ingredients can be ordered into a very large stock of combinations to which meanings can be assigned. Many human communicative systems, though not all, show duality, perhaps just because of its economy. But wherever it turns up, it seems to have been developed by analogical transfer from language. No animal communicative system on which I have information incorporates this principle. In duality, then, we may have found one genuine uniqueness of human behavior.

The second key property may be called *productivity*.[3] It is a commonplace that a speaker may say something that he has never before said or heard, and be understood without either speaker or audience being aware of the novelty. That is, we coin a new utterance on the analogy of familiar ones. We build a new utterance out of parts that have occurred in previous utterances, putting them together by patterns familiar from previous utterances. Since the audience, as well as the speaker, has had previous experience with the parts and patterns, the new combination is understood.

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[2] I have published a fuller description of duality, using the same example, (Hockett, 1955). See also Martinet (1949), where the hints implicit in linguistic discussions since the Middle Ages are explicitly spelled out for the time.

[3] According to Wells (1949) this vital property of language was first overtly pointed out by the philosopher Bertrand Russell.

Gibbons, reported to be the noisiest of our near kin, seem not to do this (Carpenter, 1940). They have a repertory of some dozen different calls, but no matter how novel may be the situation in which a gibbon finds himself, he is constrained to respond to it with one or another of this small finite set: he does not coin a new call by putting parts of old ones together.[4]

But productivity is not unique to human beings. It recurs in bee dancing (von Frisch, 1950). A worker can report a source of nectar at a location which neither she nor her coworkers has ever visited before. The mechanisms involved are quite different, but at the moment this is irrelevant.

The third property is *arbitrariness*.[5] A message means what it does *iconically* if it resembles its meaning in physical contours, or if the whole repertory of possible messages shows a geometrical similarity to the whole repertory of possible meanings. Otherwise the semantic relationship is arbitrary. Human language is almost wholly arbitrary. For example, there is no similarity between the sound of the word *dog* and the sight, sound, or smell of a dog. Nor is the difference between the sounds of the words *dog* and *cat* in any way similar to the difference between dogs and cats. In this framework, onomatopoetic forms constitute only faint traces of iconicity.

By contrast, bee-dancing is iconic. The rate of the dancing is inversely proportional to the distance from hive to source of nectar, and the angle between the line of dance and the vertical is directly proportional to the angle between the line of sight to the sun and that to the source of nectar.

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[4] A gibbon may vary the intensity and duration of a single call. This, however, finds its human counterpart not within language but in the nonlinguistic (though communicatively relevant) variations of volume, pitch, tempo, and tone-quality which accompany the linguistic ingredient in speaking. See Trager, (1958).

[5] Arbitrariness and iconicity have been the source of more trouble than any other aspect of communicative behavior, from the medieval dispute between "realists" and "nominalists" down to the present. Yet Charles S. Pierce made the distinction quite clearly in the nineteenth century; and Saussure (1916) took the arbitrariness of linguistic symbols as one of the cornerstones of his work.

The contrast between arbitrary and iconic is also exemplified by digital and analog computers. An analog computer is often beautifully adapted for a narrow function and worthless for anything else. Just so, bees can talk about nectar and hive-sites; human beings can talk about anything.

Yet arbitrariness is not a human prerogative. It characterizes gibbon calls, and probably most other vocal systems among mammals and birds (King, 1955).

The fourth property is *interchangeability*.[6] Any speaker of a language is theoretically capable of saying anything that he can understand when someone else says it. Actual limitations on this theoretical interchangeability are practical or accidental, imposed by institutionalizations of social roles or by pathologies of individual capacity. So likewise, it would seem, for gibbon calls and for bee dancing. But in some systems there is not even theoretical interchangeability. In the signalling of certain fish, male and female sticklebacks, during courtship, it is inherently impossible for roles to be exchanged (Tinbergen, 1953).

The fifth is *specialization*. An act cannot be communicative unless it involves *triggering* (Bloomfield, 1942). But any act, merely as physical activity, also has direct *energetic* consequences. When these two kinds of consequences are unrelated, we say that the communicative system is specialized. When sticklebacks are courting, an essential ingredient in one of the signals from the female is her seasonal appearance, with abdomen distended by roe. The direct consequences of this distension are obviously closely related to the trigger consequences in the male's behavior, and so the system is unspecialized. When Mrs. Jones calls out "Dinner is ready," her family are triggered into coming to the table; but the direct consequences of her action are only some minor flurries in the air, damping out with a tiny rise in the temperature of air and walls. Here we have a high degree of specialization.

The sixth property is *displacement*. We can speak about sticklebacks, or about shoes and ships and sealing wax, when none of those things are around. Our speech can be removed in space and time from what we are talking about. Bee-dancing is also displaced: the worker goes back to the hive to give her report. Gibbon calls are reported not to be displaced. If a gibbon finds food, he stands by it and emits the proper call to the rest of the band.

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[6] So far as I know, this is the first overt discussion of interchangeability and of specialization (the next property to be treated). These properties of human language come into view only when one examines certain types of animal interaction which lack them—types of interaction which many scholars would refuse to subsume as "communication" at all.

The seventh property is *cultural transmission*. A behavior pattern is transmitted culturally if it is not only learned but *taught*, and if the teaching behavior, whatever it may be, is also learned rather than genetically determined.[7] Human language is transmitted in this way, as are many other facets of human life. There is evidence for culture of a rather thin sort among the hominoid apes, and even stronger evidence in the case of waterfowl (Hochbaum, 1955), but it is not clear in either case that it is the *communicative* systems of the species that are so transmitted. Thus it may be that gibbon calls are passed down genetically, even if in some other connections gibbons, like chimpanzees, show traces of culture. For bees, we can be almost certain that genetics rather than culture is involved.

There are other universal properties of human language that might be considered, and there are certainly a great many animal communicative systems about which we are as yet in total ignorance. However, I am now going to venture a historical hypothesis based on the partial findings, fully cognizant that subsequent research may upset it.

The proto-hominoids, common ancestors of the hominoid apes and ourselves, lived some ten to fifteen million years ago.[8] It seems reasonable to suppose that they had a vocal-auditory communicative system something like that of present-day gibbons, characterized by specialization, interchangeability, and arbitrariness, properties which we may suspect were inherited from early mammalian times. It would also seem reasonable to suppose that they had a small amount of cultural transmission, though perhaps not in the domain of their most highly developed communicative system.

This proto-hominoid system was the precursor of human language. In order for it to evolve into genuine language, it had to develop four new properties. Of these, I believe the first to develop was productivity, because it is easier to explain the inception of the other three if productivity came first, and not impossible to account for the development of productivity in the absence of the other three.

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[7] Note that if we drop the requirement that the teaching behavior itself be learned, the transmission may be what the geneticists call a "maternal effect," and thus basically genetic.

[8] Based on the antiquity of *Proconsul*, who may have been our remote uncle or up-generation cousin rather than direct ancestor, but is nevertheless the nearest to a proto-hominoid so far discovered.

An unproductive system, with a finite repertory of signals, can be rendered productive only by one mechanism with which we are familiar: a mechanism known to linguists as *blending*. In blending, a speaker confronts a situation partly like two different types of situations previously experienced. He starts to say two words at once, and comes out with something different from either but partly similar to both. An example is Lewis Carroll's *slithy*, based on *lithe* and *slimy*. Conscious planning, however, is not necessary: I once hesitated between "Don't shout so loud" and "Don't yell so loud," and actually said "Don't shell so loud." A number of English words are known or suspected to have originated in this way.

I imagine that in pre-human times our remote ancestors would occasionally produce such blends, perhaps under the stress of unusual circumstances, despite the fact that their repertory of vocal signals was basically closed. Probably in thousands of cases no one responded appropriately, the blend being too radical an innovation to be intelligible. But sooner or later a few blends were communicatively successful. As soon as this happened, the closed circle was broken, and productivity was on its way.[9]

The conventions of the earlier unproductive system could be transmitted genetically, and perhaps were. But productivity, conjoined to arbitrariness, implied that members of the species were acquiring new habits from each other. It seems to me that this must automatically have called into play any capacity for culture our ancestors of the time had. Also, the more flexible communicative system had considerable survival value, and bestowed survival value on the capacity for culture (and thus perhaps also, indirectly, on increased cranial capacity).

Given arbitrariness, productivity, and cultural transmission, the young had to learn the conventions of the system before they could use it. As soon as the young were sometimes taught a habit out of the exact context in which the response would normally be evoked, circumstances were ripe for the development, or sharp increase, of displacement. The innate capacity to learn to *imitate*, outstanding in hominoids though most fully developed in Man, must have helped here.

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[9] An innovation consisting of two old signals in immediate juxtaposition counts, in the case of a closed system, as one kind of blend.

Finally, increasing productivity led to the development of duality of patterning. The exact causal connection is not clear, but I shall speculate. As the once closed system came to have a larger number of minimum meaningful elements, the latter came to be more and more similar to each other in physical contours. In time an impasse was reached. Further increase in their number would surpass the discriminatory powers of the speakers, if they had to keep the signals apart by listening to them as wholes. But this impasse could be avoided by paying attention to the smaller constituent features of sound occurrent in the wholes. It is only after the fact, of course, that we can know that the second alternative was followed. This line of reasoning derives, rather dangerously perhaps, from an observation of the language-learning process of contemporary children. Some children acquire rather large vocabularies, with excellent pronunciation; but then suddenly seem to discover that words are kept apart by constituent sounds rather than holistically; their pronunciation then deteriorates for a period, and the rate of vocabulary-learning is slowed, until they build up the necessary skills for producing and distinguishing phonemes.[10]

These successive evolutionary changes, leading to genuine language, presumably did not begin more than 10 to 15 million years ago, since our nearest non-human cousins do not show the consequences; they may have begun much earlier. They were concluded at least 50,000 years ago, and may have been completed much earlier. This second date is based on a rough estimate of the time which would have been required for all the languages of the world today to have differentiated from a single parent language, on the assumption that they are all related. I do not recommend this assumption, which is highly dubious; but it affords us our only way of directly estimating a *terminus ad quem*. Indirect inferences, based on archeological reconstructions of paleolithic life, would suggest a much earlier terminal date. Quite possibly *Pithecanthropus*, if not *Australopithecus*, shared with *Homo* the power of speech.

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[10] Again, in the history of the Chinese writing system it early became necessary, for mnemonic reasons, for new characters to be partly similar in visual shape to old ones. Here, however, full-fledged duality was never reached.

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BASIC SOCIAL CAPACITY OF PRIMATES[1]

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THE existence or nonexistence of cultural patterns in subhuman primates may best be determined by observation and comparison of the behaviors of primate groups in their native habitats. Fortunately, a considerable body of such data already exists, as exemplified by the detailed and insightful studies of primates in the wild, including the observations of Carpenter (1934) on the howler monkey, Nolte (1955) on the bonnet macaque, Zuckerman (1932) on the baboon, and last, but far from least, the magnificent unpublished studies by Washburn on three different baboon cultures.

Beyond, but not above, such data there exist laboratory studies, both experimental and observational, on more limited aspects of primate behaviors, the kinds of specific traits and capacities which it is reasonable to assume must exist if cultural acquisitions are to be passed down from generation to generation in the wild. In this regard, we may be interested in both the kind of capacities making possible existing human cultures and the kind of developing capabilities that may have gradually led to the formation of our own cultural environment.

Culture is dependent upon the ability of an animal within a social group to acquire from another group member information previously learned by the latter member. To obtain such information it is essential that the animal, particularly the young animal, observe other members of its social group and duplicate their behaviors either explicitly or implicitly.

It has long been known that monkeys in a relatively free situation do observe other monkeys and consistently duplicate their behaviors. The early literature was summarized by Watson in 1914, and we quote limited sections: "With Kinnaman we found that if one monkey discovered a hole and peeked into it, another would generally push him aside and peek into it in his turn. This was observed many times. More recently we confirmed these observations many times in the case of a

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[1] The author's research reported at the end of this paper was supported in part by Grant M-772, National Institutes of Health, and in part by funds received from the Graduate School of the University of Wisconsin.

mother and baby. One day the baby left the mother's arms and peered under a sill at a brass lock which could be seen from his viewpoint but not from hers. He pushed his arm through and attempted to pull the lock forward. The mother left off eating, came and adopted the same position and reached her arm through and attempted to grasp the lock. The two acts were identical in technique" (pp. 284-285).

Anyone who has attempted to tame and adapt groups of monkeys to the experimenter or to an apparatus knows that the behavior of the tamer subject greatly influences the behavior of those less tame. We again quote representative observations from Watson: "Two of the monkeys were quite friendly with each other. J was an adult monkey and B a young one. In a short time an attachment sprang up between these monkeys which persisted for several years. . . . When J went to one part of the cage, B followed. If, while sitting on the shelf, a pan of water or bowl of milk was placed upon the ground, B would not come down to drink if J did not precede him. J formed the habit of jumping on to the experimenter's shoulder when he entered the cage and called. B formed the same habit, but if J for any reason refused to come, B refused also" (p. 283).

Dr. Mason, at the Wisconsin Primate Laboratory, has made extensive and detailed observations under controlled conditions of the behavior of infant monkeys separated from their mothers at birth and raised subsequently in individual cages. Within the first month of life there developed in these infants strong tendencies to observe the exploratory-manipulatory behaviors of the other member of the test pair, and subsequently to engage in and esentially duplicate these activities. This imitative responsiveness becomes stronger with age, clear-cut individual and pair differences develop, and individual imitative responsiveness becomes influenced by peer status. Dominant animals both observe and duplicate the behavior of the subordinate, whereas subordinate animals tend to observe, but not duplicate, the behavior of the dominant member of the pair, or to defer duplication of this behavior until the dominant animal has left the area.

In Watson's 1914 review he concluded that there was no evidence in primates of imitative learning involving more complex tests, such as problem-box devices and tools. However, Warden and Jackson in 1935 tested 15 rhesus monkeys in the Warden duplicate-cage imitation apparatus and obtained positive evidence of imitative learning on problem-box devices, ranging in difficulty from pulling a chain to the operation of two latches. The opportunity for imitative learning was provided by illuminating the cage of the trained monkey, which then solved the particular problem five times, usually within a 30-second period. The light was then turned on in the imitator's cage, and it was allowed 60 seconds to solve the observed problem. Six such opportunities were provided on each of four tasks. Successful imitation occurred on 46% of the tests, and eliminating 6 subjects that did not adapt to the situation raised the percentage of successful imitation trials to 72. The most successful animal imitated in 23 of the 24 tests presented. Subsequently, Warden, Fjeld, and Koch (1940) conducted similar tests on cebus monkeys and obtained similar results.

There are many formal psychological experiments in which the animal can solve the problem only by observing the behavior of the experimenter. This is true, for example, in the delayed-response test run by the direct method. To solve the problem, the animal must observe the foodwell or cup in which the experimenter places the food before covering it and the unbaited well with the identical stimuli, thus initiating the delay period. Primates readily adapt to such procedures, and so, for that matter, do other animals, including cats, dogs, raccoons, and even rats. Harlow (1944) trained a monkey to solve a series of discrimination problems when the only cue to the correct stimulus was the fact that it had been tapped by the experimenter. If primates can solve formal laboratory problems when the only clues to solution are those obtained from observing the experimenter, it seems only reasonable to assume that they can solve such problems when the cues are obtained by observing or imitating a fellow member of their species.

The first indication that subhuman primates might solve a discrimination problem merely by watching the solution of the same problem by another animal was obtained by Crawford and Spence (1939), who used chimpanzees as subjects. Perfect learning in this situation was obtained in one of 11 subject pairings, and partial learning was found in two other cases before the imitator showed clear behavioral indications of loss of interest.

Miller and Murphy (1956) have recently compared the learning of discrimination and oddity problems by rhesus monkeys tested individually and in pairs, and they found that social interaction improved performance significantly on a series of oddity problems and significantly improved the performance of previous discrimination tasks. Data from a study by Darby and Riopelle (1958) give even more striking proof of the ability of primates to learn discrimination problems entirely on the basis of observation of a partner. They used an adaptation of the

Crawford-Spence technique in which stimulus objects were placed on a tray between two opposed animal cages and the subjects could observe each other and the test situation. A total of 1000 problems was given, on half of which each animal served as demonstrator and on the other half as observer. Each problem lasted only four trials, one free-guess trial by the demonstrator and three test-trials by the observer. The imitators became progressively more proficient in solving problems after a single observational trial. Thus, the imitator learned to solve successive problems with progressively greater facility, *i.e.*, to make very significant progress toward the formation of a discrimination learning set. In view of the fact that the formation of discrimination learning sets is by no means a simple task for the subhuman primate, a truly amazing capability of imitative learning was demonstrated. Indeed, not only could the observer select the same object that was chosen by the demonstrator when it covered food but the animal could also take into account errors made by the demonstrator.

There is reason to believe that there are subtle factors influencing imitative learning by primates, human or subhuman. It is obvious that the nature of the social interactions already existing between members of the test pair is of primary importance. It is more than possible that the conditions existing in nature have been more favorable in this regard for producing imitative learning than have the conditions in the laboratory. Mother monkeys, for example, may have far more insight into the importance of these variables than do experimental psychologists. Being now able to identify some of these variables, we hope that both more spectacular and more illuminating experimental data will be provided by comparative psychology laboratories in the future.

Although the existent experimental data give us no evidence that anthropoid apes have greater capacity to profit from imitative learning than do Old World monkeys, there is enough observational evidence to evoke at least mild skepticism about this point. Köhler's (1925) beautiful pictures showing intent observation by single, or even multiple, chimpanzees while a single member of the clan works on the solution of a multiple-tool problem, have never been duplicated with monkeys. It is more than possible that the chimpanzee's capabilities along these lines have been even less thoroughly exploited than have the monkey's. An extremely interesting set of observations on imitative behavior in a home-reared chimpanzee was made by Hayes and Hayes (1957) on their adopted chimpanzee, Vicki. From the age of 17 to 34 months, Vicki was given occasional training on a series of tasks intended to establish a learning set for learning by imitation. They rewarded Vicki for imitating actions accompanied by the command, "Do this," training her to imitate actions that had no intrinsic play value. A list of 70 items, ranging from simple tasks such as hand clapping and head patting, to more difficult problems, including a three-lever problem box and a key-and-padlock task. The Hayeses were in general quite successful in this effort, and there can be no question but that a set was established in Vicki to attempt imitation upon request. The attempts were made in many cases after delay and in some instances even under conditions of subterfuge. The attempted imitation in no way implied successful imitation, particularly for difficult problems. Some failures were attributable to motor limitations, and some were doubtless dependent upon learning limitations. But the heart of the problem was not that of teaching the chimpanzee to solve all problems by imitation, but to teach the subject to try to imitate, and in this the Hayeses were most successful.

The existence of a human-type culture is dependent upon learning capability and the capacity to transmit this learning to subsequent generations. Without language the capacity to transmit previous learning is dependent upon either such forms of imitative learning as we have described, or a simpler type of learning in which the infant follows the group, and the movement and behavior of the group are such as to place the infant repetitively in the situations where the infant learns by direct experience the danger responses or affectional responses essential to individual and group survival.

Laboratory researches of the last quarter-century have demonstrated that both chimpanzees and rhesus monkeys (and presumably other apes and other monkeys) possess a heretofore unsuspected ability to master the general kinds of problems that seem to characterize human intelligent behavior. It is difficult to describe these problems briefly and in a nontechnical manner, but we may indicate their complexity by pointing out that many of them, such as the complex oddity and matching problems, are similar in nature to test items in standard intelligence tests for children, and in diagnostic tests for brain-damaged adults. Others of these tests, including the development of rather abstract concepts such as triangularity, color, and number, are tasks which may prove troublesome for the preschool or primary school child. All data we have to date seem to indicate that the subhuman primates learn such problems with a facility quite beyond that of any other subhuman mammals or other classes of animals.

Furthermore, there appears to be another kind of learning ability essential to any kind of complex learning or thinking which is, or approaches, being the private property of the primate order. This is the ability to effectively transfer the learning of a particular problem to the learning of another problem of the same kind or class, a phenomenon described as the formation of learning sets. If, for example, we train a macaque monkey for 6 trials on each of a series of 100 discrimination problems, the monkey makes progressive improvement on these problems and eventually solves any of the problems with perfect insight. This kind of problem can apparently be solved by all monkeys and by most human beings. A study by House and Zeaman (1958) indicates that it is beyond the capacity of human imbeciles, and similar studies have obtained camparable results with other retarded people.

We have no proof that the ability to learn abstract problems of this type has contributed to the kinds of subhuman cultures so admirably described by Washburn in his field study of baboons, but the ability to learn multiple and even more complex problems of the kind described is an essential prerequisite to the evolution of our own human cultures.

There has been speculation that one of the factors that has contributed to human culture is the long period of infancy or dependency and the ability of the human infant to learn during this period of time. The anthropoid apes and the monkeys also undergo relatively long periods of infancy or dependency, and it is now quite clear that they have developed relatively great capacity to learn during this time. If we assume that the human being is dependent until attaining or approaching adolescence, we may note that this is the time at which learning capability approaches a maximum, and the human animal has been an effective learner for approximately ten years.

We have no definitive data on the period of dependency of the macaque (or other) monkey, but observational evidence suggests a period of two or three years, and we may be approaching complementary data from experiments at the Wisconsin Primate Laboratory. Though this period is short in comparison with that of man, the macaque becomes an effective learning machine much more rapidly than does the child. The macaque can learn conditioned paid responses in the first two or three days of life, simple spatial reflexes by 15 days of age, black-white discriminations by 15 days of age, and learning sets by the end of the first year. Thus, the monkey, like the human being, is a capable and efficient learning animal during this period of developmental dependency. Equally precise data do not exist for the chimpanzee (or any other anthropoid), but its position both in terms of period of dependency and maturation of learning capacities is without doubt intermediate between that of monkey and man. This interrelationship between dependency and abstract learning capabilities has failed to give either monkeys or apes a human-type culture, but it may well be a function that has proved important in the evolution of man's capacity for culture.

The evolution of culture, particularly human-type cultures, which involve complex interrelations among large groups of people, is only possible if there exist positive cohesive forces stemming from innate affectional factors which may be modified, generalized, and enhanced by learning. The existence and operation of these affectional factors has been described and assigned proper importance by Carpenter, Zuckerman, and Washburn for the howler monkey, macaque, and baboon, and by Kohler (1925) and the Yerkes (1929) for the chimpanzee. These affectional responses are above and beyond those associated with sexual behavior, and important as these latter behaviors may be, I believe we may safely assume that sex alone is not enough to account for the cohesive forces essential to the development of human cultures.

The description and enumeration of the multiple patterns of affectional responses of monkeys and apes can be accomplished more efficiently and comprehensively by the study of the free-ranging primates than by the study of the primate as a laboratory animal. The contribution which the laboratory may make, above and beyond naturalistic study, is the analysis of the nature and, to a limited degree, the development of these affectional forces. At the Wisconsin Primate Laboratory we have initiated a series of studies designed to determine the various innate affectional factors and to determine the importance and role of these separable factors. We do this by separating the infant from the mother at birth, and then raising these orphans on artificial and inanimate pseudomothers whose stimulus qualities—such as body texture, presence or absence of lactation, motility, vocalization, and presence or absence of appendages—can be controlled and separably manipulated.

In plate I, top, we show two of these pseudomothers described arbitrarily as good and bad pseudomothers. The good pseudomother is made of a block of wood covered by sponge rubber which is sheathed by a terry-cloth skin. The bad pseudomother is made of hardware cloth, a substance entirely adequate to provide support and nursing capability. In one of our test situations, the infant is free to leave its living cage at any time and contact either pseudomother. For half the infants the bad pseudomother lactates and the good pseudomother does not; for the other infants this condition is reversed. In figure 1 we present data showing the total time spent on the cloth and wire pseudomothers under the two conditions of feeding. These data make it obvious that body texture is a variable of overwhelming importance in the development of affectional responses, and lactation is a variable of negligible importance whether this relates to innate responsiveness to the pseudobreast or to any learned or derived motives built upon the response of nursing. Indeed, it is possible that the only function of nursing as an affectional variable is that of insuring frequent body contact. Body surface is, of course, a

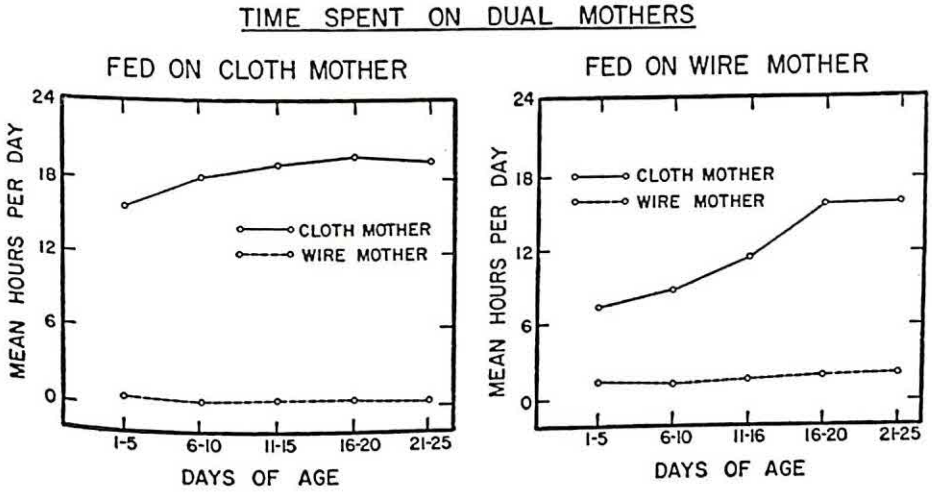


FIG. 1. TIME SPENT ON DUAL MOTHERS.

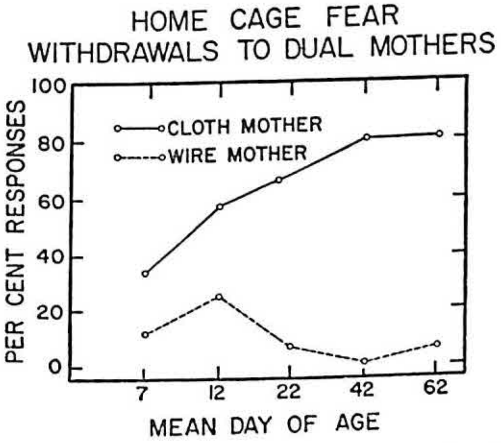


FIG. 2. HOME CAGE FEAR WITHDRAWALS TO DUAL MOTHERS.

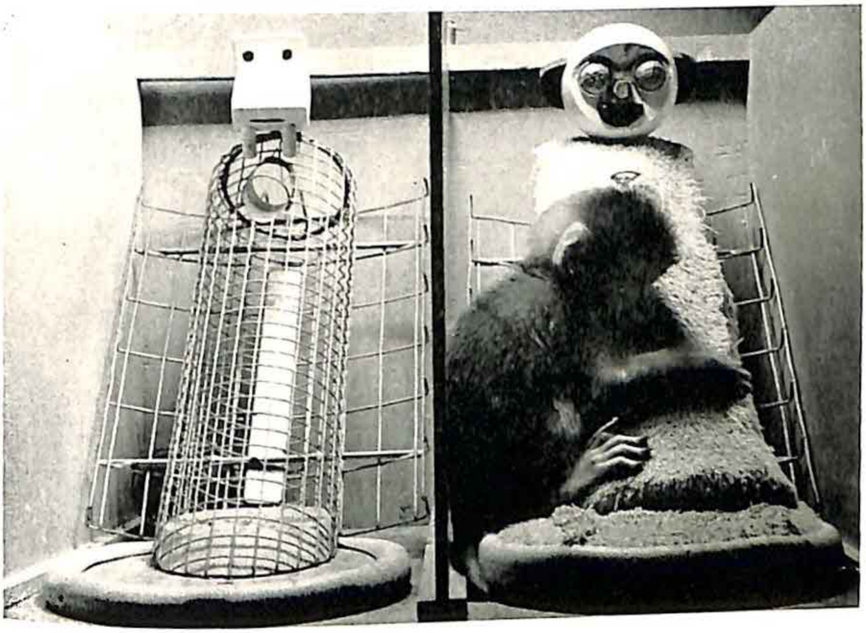
variable with far greater potentialities for primary stimulus generalization than is a lactating breast, since all monkeys at all times have a body surface, and no monkey at all times has a lactating breast.

One function of the mother or mother surrogate is to provide a haven of safety for the infant in times of fear or danger. The frightened or ailing child clings to its mother, not its father, and this selective responsiveness in times of distress, disturbance, or danger may be used as a measure of the strength of affectional bonds. We have tested this kind of differential responsiveness by presenting our infants with various fear-producing stimuli such as this moving toy teddy bear shown in plate I, bottom. The data on differential responsiveness are presented in figure 2, and it is apparent that the cloth pseudomother is highly preferred over the wire one, and this differential selectivity is enhanced by age and experience. The variable of nursing appears to be a variable of absolutely no importance in this situation as indicated by our relatively scanty data at the present time. Contact comfort is one skin game which is essentially good and not evil or improper. If the infant orphans are raised on a single cloth mother they invariably seek her succor in times of terror. If the infant orphans are raised on a nursing, wire mother, they never seek her succor in times of fear, but, instead, throw themselves prostrate on the floor, wrap their head in their arms, and scream in terror.

Pseudomother-raised monkeys, and control monkeys of comparable ages, raised for 14 days on a cheesecloth diaper and henceforth in a bare wire cage, were introduced into the strange environment of a room measuring 6 feet by 6 feet by 6 feet and containing multiple stimuli known to elicit curiosity-manipulatory responses in baby monkeys. All the subjects were placed in this situation twice a week for 8 weeks with no pseudomother present during one of the weekly sessions and the appropriate pseudomother, the kind which the experimental infant had always known, present during the other. A cloth diaper was always available to all monkeys during all sessions.

The animals raised on the good cloth pseudomother always rushed to the mother when she was present and clutched her with loving endearment. Then without exception, they began exploring the room and manipulating the stimuli, and some even brought the stimuli to or around the mother, as if they wished her to share their treasures. The behavior of the infant raised on the good pseudomothers was quite different when she was absent from the room. Emotional indices such as vocalization, crouching, rocking, and sucking increased sharply, and many of the animals would rush to the center of the room where the

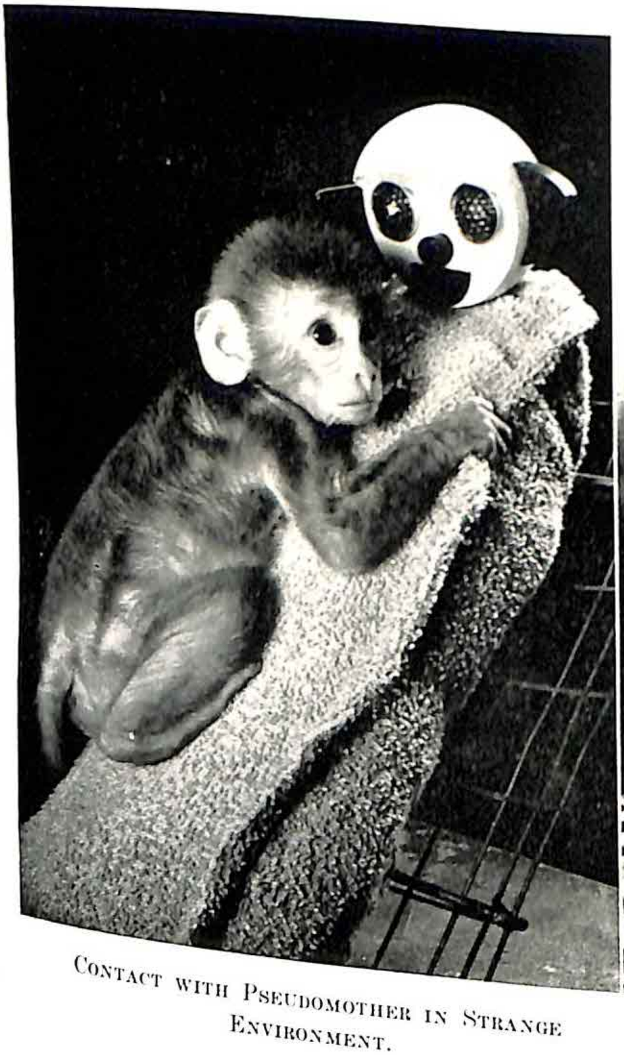
PLATE 1.



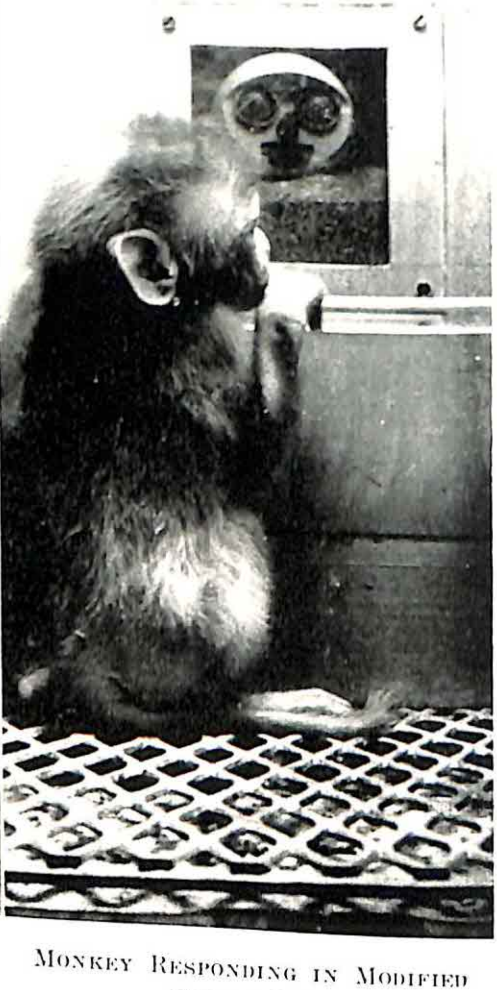
WIRE AND CLOTH PSEUDOMOTHERS.

REPRESENTATIVE FEAR-PRODUCING STIMULUS.

PLATE II.



CONTACT WITH PSEUDOMOTHER IN STRANGE ENVIRONMENT.



MONKEY RESPONDING IN MODIFIED BUTLER BOX.

mother was always positioned, and then run rapidly from object to object, screaming and crying all the while. Continuous, frantic clutching of their own bodies was very common, even when they were not in the crouching position. These monkeys frequently contacted and clutched the cloth diaper, but this action never brought surcease to their sorrow.

The behavior of the control monkeys and those raised on the wire pseudomothers was similar under all test conditions. They simply froze in the strange situation and made no attempt to contact the wire pseudomother or the diaper or any test object during any part of any test session. The experimental and control infants ranged in age from 7 to 108 days, and there was no responsiveness to the diaper or the wire mother which could increase or decrease. These data suggest that man cannot live by milk alone.

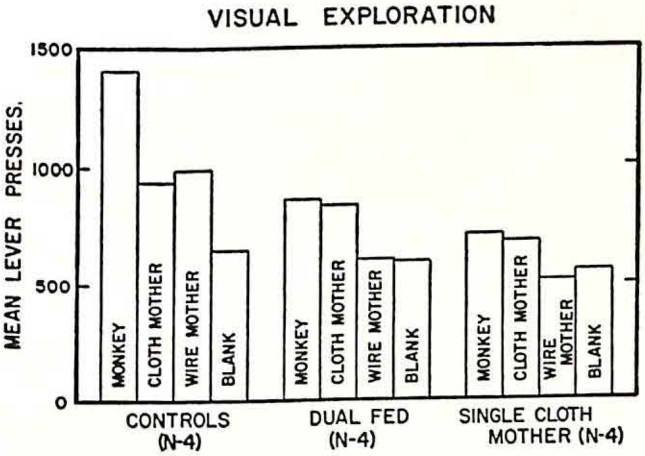


FIG. 3. DIFFERENTIAL VISUAL-EXPLORATION RESPONSIVENESS.

A number of years ago Butler (1953, 1954) demonstrated that monkeys enclosed in a box would open and reopen a door for hours on end, for no other motivation than of viewing. Furthermore, he showed selectivity in rate and frequency of door opening to stimuli of differential attractiveness. We have utilized this apparatus to measure in a positive manner the strength of affectional responsiveness in our infants, as illustrated in plate II, right. The data of figure 3 show equally strong responding to the cloth pseudomother and another infant monkey and no greater responsiveness to a wire pseudomother than to a blank cage. These data are again independent of the kind of mother which nursed the infant.

The fact that the comfort of contact is the variable of prime importance in the development of filial affection is no more surprising than the finding that good pseudomothers, as inanimate as if they were made from a block of wood, which they were, elicit such strong and persistent affectional responses. The infant seeks them when theatened, feels secure in their presence, views them with loving adoration, learns to solve puzzle boxes and discrimination problems to attain them, and grieves in their absence. Our oldest pseudomother-raised monkeys are now 6 months of age, and their responsiveness to their mothers seems to be increasing rather than decreasing. We are now beginning studies to determine how strong and how persistent are the affectional responses when the infant and pseudomother are separated. Our data are limited, but we are certain that affectional responses once formed are very persistent, perhaps as persistent as responses conditioned to pain. It appears that the affectional responses to the pseudomothers generalize adequately to real monkeys, although we have not yet subjected this problem to experimental analysis. We are beginning studies that will enable us to compare the strength of affectional responses to pseudomothers and restrained real mothers, lactating and nonlactating, and to restrained real fathers.

Anthropologists and evolutionists have emphasized the importance of freeing the forelimbs from locomotor functions for activities of exploration, tool construction, and general language. But another important function of arms is their capability of affording intimate and maximal contact comfort to the infant, and it is possible that this manual maximizing of contact comfort for long periods of time, time during which the infant has developed excellent learning capabilities, has contributed to the strong and enduring individual affectional attachments important to the development of any complex culture.

Our experiments on the nature and development of affectional responses in rhesus monkeys are not designed to attack directly the problem of the evolution of culture within the primate order, but it is our hope and faith that they provide very useful information concerning a kind of response of vast importance to the social behavior of all animals in all societies and cultures.

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THE SOCIAL LIFE OF MONKEYS, APES AND PRIMITIVE MAN[1]

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INTRODUCTION

THIS study compares societies of infrahuman primates with the most rudimentary of documented human social systems. The objectives are to describe general trends in primate social organization leading to human society, and to delineate the major advances of the latter, cultural society, over precultural society.

For comparative materials, we rely on field studies of monkey and ape social behavior, supplementary observations of these animals in captivity, and ethnographic accounts of simple hunters and gathers. The quality and quantity of published studies of free ranging subhuman primate societies do not provide comfortable support for weighty generalizations. Aside from the anecdotal literature, we have only Carpenter's accounts of spider monkeys, rhesus, howling monkeys, and gibbons; Nissen's chimpanzee material; Zuckerman's observation of baboons (perhaps biased by his captivity studies); the as yet incomplete reports on the Japanese monkey; and some peripheral notes on the African red-tailed monkey by Haddow, and on the gorilla by Schwab.[2] *Considering this, our interpretations of subhuman primate social behavior are entirely provisional.* The data on primitive food gathers are more abundant. We include in our comparison the following primitive societies: Australian Aborigines, Tasmanians, Semang, Andamanese, Philippine and Congo pygmies, Bushmen, Eskimo, Great Basin Shoshoni,

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[1] I thank Professors M. H. Fried, F. P. Thieme, S. L. Washburn, E. R. Service, J. N. Spuhler and L. A. White for their useful criticisms of an earlier version of this paper. I am especially indebted to White's articles touching subjects dealt with here (White, 1949, Chapters II, III, VI, XI). Needles to say, the above named do not necessarily agree with all statements contained herein.

[2] The following provide the field data on subhuman primates used in this paper: Carpenter, 1934; 1935; 1940; 1942c; Collias and Southwick, 1952; Haddow, 1952; Hooton, 1942 (report of Schwab's observations); Imanishi, 1957; Nissen, 1931; and Zuckerman, 1932. Unless quoting or making special points, these works will not be specifically cited hereafter.

Naskapi, Ona and Yahgan.[3] It is asumed that these societies parallel early cultural society in general features. This is simply an assumption of order and regularity. The technologies and low productivity of modern hunters and gathers resemble the archaeologically revealed productive systems of early cultures. Granting that a cultural social system is functionally related to its productive system, it follows that early human society resembles rudimentary, modern human society. This reasoning is supported by the large degree of social similarity among the present hunters and gathers themselves, despite the fact that some of them are as historically distant from each other, as separated in contact and connection, as the paleolithic is separated from modern times. Further, simply because many food gatherers have been driven into marginal areas, they are not thereby disqualified from consideration. There still remain strong social resemblances between marginal peoples, such as Bushmen, Ona, and Eskimo, and those found in isolated, but otherwise not ecologically marginal areas, such as many Australian groups and the Andaman Islanders.

A comparison of subhuman primate and primitive society must recognize the qualitative difference between the two (see White, 1949: Chapter III). Human society is cultural society; the organization of organisms is governed by culture traits. The social life of subhuman primates is governed by anatomy and physiology. Variations in human society are independent of, and are not expressions of, biological variations of the organism. Variations in primate society are direct expressions and concomitants of biological variation. Nissen writes: ". . . with one notable exception the phylogenetic course of behavioral development has been gradual . . . it has been a continuous affair, proceeding by quantitative rather than qualitative changes. The one exception is that which marks the transition from the highest nonhuman primates to man. . . . At this point a new 'dimension' or mode of development emerges: culture." (Nissen, 1951: 426).

It follows that assertions of specific phylogenetic continuities from anthropoid to primitive society must be summarily rejected, such as Yerkes' suggestion that delousing among primitives is a genetic survival of primate grooming, an activity which, Yerkes writes, also led to: "tonsorial artistry, nursing, surgery, and other social services of man." (Yerkes, 1933: 12). In the same vein is Kempf's identification of the presenting behavior of a subordinate rhesus monkey with human prostitution (Kempf, 1917, cf. Miller, 1931). Furthermore, the terminology of cultural social and political organization should be disavowed when describing infrahuman primate society. The cultural anthropologist justifiably shudders when he reads of "clans," "communism," and "socialism," among howler monkeys, or, "despots," "tyrants," "absolutism," and "slavery," among baboons.

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[3] The following provide the field data on hunters and gathers used in this paper: Birket-Smith, 1936; Bleek, 1928; Boas, 1888; Cooper, 1946a; 1946b; Elkin, 1954; Forde, 1934; Gusinde, 1955; Leacock, 1954; 1955; Lothrop, 1928; Man, 1885; Putnam, 1953; Radcliffe-Brown, 1930-1931; 1948; Rink, 1875; Roth, 1890; Schapera, 1930; Schabesta, 1933; n.d.; Sharp, 1934-1935; Spencer and Gillen, 1927; Steward, 1938; Vanoverbergh, 1925; Warner, 1937; and Weyer, 1932. Unless quoting or making special points, these works will not be specifically cited hereafter.

The determinants of sociability are different in cultural and precultural society. We find useful Zuckerman's contention that social organization in general is based upon, "three main lines of behavior—search for food, search for mates, avoidance of enemies." (Zuckerman, 1932: 17). Of these factors, the sexual one appears to be primary in the genesis of subhuman primate society: "The main factor that determines social groupings in subhuman primates is sexual attraction." (*Ibid*: 31). "The emergence of this feature [*i.e.*, of continuous sex activity] into prominence in their behavior has created primate society." (Chance and Mead, 1953: 415). It was the development of the physiological capacity to mate during much of, if not throughout, the menstrual cycle, and at all seasons, that impelled the formation of year round heterosexual groups among monkeys and apes.[4] Within the primate order, a new level of social integration emerges, one that surpasses that of other mammals whose mating periods, and hence heterosexual groupings, are very limited in duration and by season. Certainly, defense against predation is also a determinant of subhuman primate sociability (Chance, 1955), but it, and the search for food, appear to be secondary to sex. The influence of sexual attraction in promoting solidarity among subhuman primates has been noted in the field. Carpenter has observed of the howlers: "With repetition of the reproductive cycle in the female and with uninterrupted breeding throughout the year, the process of group integration through sexual behavior is repeatedly operative, establishing and reinforcing intersexual social bonds." (Carpenter, 1934: 95; for a common alternate view, see Yerkes and Yerkes, 1935: 979.)

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[4] The Japanese monkey, *Macaca fuscata*, northernmost of all subhuman primates, is reported to have a breeding season (Imanishi, 1957).

Sexual attraction remains a determinant of human sociability. But it has become subordinated to the search for food, to economics. A most significant advance of early cultural society was the strict repression and canalization of sex, through the incest tabu, in favor of the expansion of kinship, and thus mutual aid relations (Malinowski, 1931; Tylor, 1888; White, 1949: Chapter XI). Primate sexuality is utilized in human society to *reinforce* bonds of economic and to a lesser extent, defensive alliance. "All marriage schemes are largely devices to check and regulate promiscuous behavior in the interest of human economic schemes." (Miller, 1931: 402). This is not to underestimate the importance of primate sexuality in determining certain *general* characteristics of human society. If culture had not developed in the primate line, but instead among creatures practicing external fertilization, marriage and rules of exogamy and endogamy would not be means of establishing cohesive groups in cultural society. But, in the transition from subhuman to human society, cooperation in subsistence activities became the dominant cause of solidarity, avoidance of enemies a secondary cause, while sex became simply a facilitating mechanism.

These propositions are best documented by detailed consideration of primate and primitive society, wherein the differences in causes of sociability will be seen to pervade the comparison, and to turn generic continuity into specific discontinuity.

SUBHUMAN PRIMATE AND HUMAN PRIMITIVE SOCIETIES

Territoriality is one of a number of common features of subhuman primate and primitive social behavior. It is also general among lower vertebrates—perhaps it is a universal characteristic of society. Territoriality arises from competition over living conditions, and has the selective advantage of distributing the species in its habitat so as to maintain population density at or below its optimum (Allee, *et al.*, 1949: 411 f; Bartholomew and Birdsell, 1953: 485).

Territorial relations among groups of subhuman primates of the same species are generally exclusive. Except for the few animals that are driven out of one group and may attach themselves to another, primate societies are usually semi-closed societies. (Carpenter, 1942a: 187). Each horde has a focus of favorite feeding and resting places to which it is often deflected by contact with other groups. Contact between groups of the same species at territorial borders is generally competitive and antagonistic, sometimes violently so. Subhuman primate groups apparently have little tendency to federation. They, "*do not have supergroup social mechanisms. . . .* Kinship relations are not operative and inbreeding is the rule rather than the exception. . . ." (Carpenter 1954: 98). Carpenter finds the origin of intergroup cooperation characteristic of primitive tribalism, "difficult to trace in subhuman primates which I have studied." (1940: 163).

Primate territorial relations are altered by the development of culture in the human species. Territoriality among hunters and gatherers is never exclusive, and group membership is apt to shift and change according to the variability of food resources in space and time. Savage society is open, and corresponding to ecological variations, there are degrees of openness: 1. Where food resources are evenly distributed and tend to be constant year to year, territory is clearly demarcated and stable, and, considering the nucleus of males, so is group membership. The Ona and most Australian groups are representative of this condition. Stability of membership is effected through customary rules of patrilocal residence, which is usually coupled with local exogamy. Patrilocal residence confers the advantages of continuity of occupation for hunters in areas with which they are familiar. 2. Where food resources are evenly distributed during some seasons, and variably abundant during others, exclusiveness of territory and membership are only seasonal. Local groups of Central Kalahari Bushmen, for example, remain fixed in territories focused around water holes during the dry season; whereas, in the rainy reason, such groups mingle and hunt together. Similarly the Semang have seasonal territories fixed by the distribution of the durian tree; after the harvest, territories and exclusive groups dissolve, to be reconstituted at the next durian season. Under these conditions, patrilocal residence and local exogamy are preferred, but not strict, rules, and local endogamy and matrilocality occur. The Andaman Islanders with relatively fixed territories, a tendency toward local exogamy, and no residence rule, and the Yahgan, who tend to be locally exogamous, patrilocal, and territorially exclusive, may also fall into this ecological type. However, data available for classification of these groups are inadequate. 3. Finally, there are food gatherers among whom rules of territoriality are *de facto* nonexistant, and local group composition highly variable. These occupy areas where food resources vary in local abundance seasonally and annually. Families coalesce and separate *ad hoc* corresponding to accessibility of supplies. Postmarital residence may be in the group of either spouse, and band aggregates are agamous—there are no rules.

The Great Basin Shoshone, the Eskimo, and the pre-fur trade Naskapi fall into this category.[5]

Territoriality among hunters and gatherers is sometimes maintained by conflict. There appears to be a direct relationship between intensity of intergroup feud and exclusiveness of territory and membership. Thus trespass is strongly resented and interband feuds are relatively frequent among Australians and the Ona; whereas, where territoriality is weak, as among Eskimos and Shoshoni, the concept of trespass is naturally poorly developed, and fighting consists of squabbles between particular families. However, in all cases the outcome of trespass depends on the previous relations between neighbors, and these are usually *friendly*. Even among the Australians, adjacent groups would be allowed to hunt in a band's territory if in need; there is "no constant state of enmity" between neighbors (Spencer and Gillen, 1927, i: 58). Everywhere, no matter how strict the rules of territory, constant visiting between bands prevents the development of closed groups. And everywhere exclusiveness is easily broken down if there is some food windfall, or if food is differentially abundant in adjacent locales.

Hunters and gatherers live in relatively open groups between which relations are usually friendly; infrahuman primates of the same species live in relatively closed groups between which relations are usually competitive. The invention of kinship and the incest tabu of cultural society are responsible for this difference. Through marrying out, friendly, cooperative relations are established between families. When exogamy can be extended to the local group, cooperation between bands is effected (Tylor, 1888; White, 1949; Chapter XI). Intermarrying Australian bands are described as, "*a family* of countries bound together by those sentiments which function between members of a family and its near relations." (Elkin, 1954: 81; emphasis ours). It is the kinship ethic of mutual aid that permits populations of hunters and gatherers to shift about according to the distribution of resources. Kinship is thus selectively advantageous in a zoological sense; it permits primitives to adjust to more variable habitats than subhuman primates (see Carpenter, 1955: 93). Moreover, the kinship relations between groups, and the ceremonies and exchanges of goods that frequently accompany interband meetings, give rise to a further social development: tribalism. Common custom, common dialect, a name and a feeling of unity are created among otherwise independent groups. The stage for further political evolution is thereby set.

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[5] Philippine and Congo pygmies are presently of the first type: rigid territory, patrilocal, exogamous. However, both live in symbiotic, subservient relation to agricultural peoples; and, at least for the Congo Twides, territorial exclusiveness is clearly a function of boundaries between patron Bantu villages (Putnam, 1953; Schabesta, 1933; Gusinde would disagree, 1955: 23). The Heikum Bushmen, also in symbiotic, subservient status to patrilineal Hottentot and Bantu groups, are another instance of the same thing. Unlike other Bushmen, the Heikum live in well defined territories and practice strict patrilocality and local exogamy (Schapera, 1930: 34-35, 38, 83, 94f). Leacock's (1955) Naskapi studies suggest that bilocal, nonterritorial, unstable bands commonly become so formalized under outside influences of this general sort.

We turn now to the internal organization of subhuman primate and primitive human societies.

The subhuman primate horde varies in size from an average of four animals among the gibbon to several hundred baboons (Carpenter, 1942a). Group size is not correlated with suborder differences, except that great ape hordes are generally at the lower end of the primate range; orang-utan groups are apparently as small as gibbons' and chimpanzees average 8.5 per group. The horde may remain together at all times, or may disperse, during daytime feeding, into segments of various constitution—mating groups, female packs, male packs—concentrating at night resting places. Howling monkeys typically travel together; spider monkey and baboon groups are instances of segmented hordes.

With the exception of the gibbon, the primate horde characteristically contains more adult females than adult males. In observed wild groups the ratio ranges from nearly 3:1 for howlers, to 1:1 for gibbons. The ratio for spider monkeys, 1.6 females per male, is probably near the average for the primates. The usual inequality apparently reflects the degree of dominance and competition among males for female sexual partners.[6] There are almost always unmated males, either periphereally attached to heterosexual groups, or existing outside the horde.

We have argued that sexual attraction is the primary cause of subhuman primate sociability. Indeed, in many cases the entire horde is a single reproduction unit or mate group. But there are significant species variations in the constitution of hordes and mate groups. There appears to be a progressive development within the primate order from promiscuous relations within the group to the establishment of exclusive sex partnerships, one of which comprises the nucleus of a horde. Since the young remain attached to adult females throughout, the highest forms of primate mate groups resemble the elementary human family in composition (Carpenter, 1942a: 186; Chance and Mead, 1953: 418; Yerkes and Yerkes, 1929: 566). The emergence of exclusive, independent mate groups takes the following steps: 1. Among New World howler and spider monkeys observed in free-ranging conditions, the only stable relations within a horde are between females and their young. Only when females are in the oestrus period of the menstrual cycle do they leave the female-offspring pack and become attached to specific males, and then not exclusively, but to several in succession. The mated pair is a temporary, non-exclusive unit. 2. Old World monkeys develop more permanent sex partnerships. Rhesus shows the trend toward exclusiveness. Again in rhesus, the female-young pack is a separate unit, and sex partnerships are only temporarily established while a female is in heat. However, for every female, the succession order among her male partners corresponds to their dominance position. Therefore, dominant males have all females in oestrus, while subordinate males are excluded from some when there are not enough to go around. The mate group of the Japanese monkey, in the same genus as rhesus, is very similar. 3. The baboon mate group is a simple extrapolation from rhesus. The steps involved are: the exclusion of subordinate males from sexual relations with females, and the development of constant association between a dominant male, females and young. The baboon mate group is a permanent, exclusive relationship between a dominant male and his several females, the young following their mothers. Subordinate males may remain attached to the group on its fringes, or form unisexual bands. The baboon horde consists of several such mate groups and male bands, each relatively independent. 4. The ape horde tends to be composed of a single, independent mate group of the baboon type. In the gibbon, this consists of one male, one female plus young. The evidence from the other anthropoids is not conclusive; however, gorilla, orangutan and chimpanzee hordes apparently consist of a single mate group of one male, two or more females and their young, and perhaps subordinate males.

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[6] Washburn has suggested (personal communication) that the unequal ratio may in good part be due to the faster maturation rate for females.

The emergence of exclusive, permanent mate groups among higher primates is explained by the progressive emancipation of sexual behavior from hormonal control running through the order (Beach, 1947). In monkeys, copulation outside the female's fertile period is relatively rare. In apes—more commonly so, in man—sex is freed from hormonal regulation, being subject instead to cortical and social control.[7] The oft-made alternative assertion that the development of the family is due to increasing duration of infant dependency is not supportable. With one minor exception, subhuman primate males are never significantly involved in rearing the young, save in retrieving the fallen, and indirectly as group defenders. The exceptional case of the male Japanese monkey that rears the older infant if a female has two in succession is not significant here, since there is no family-type group involved. This behavior was observed only in one of a number of hordes of Japanese monkeys, and is entirely unique among subhuman primates. Baboon and rhesus males are known to have killed young in the course of sexual attacks upon their mothers. A long dependency period cements mother-offspring relations but not necessarily father-mother-offspring relations. Only when there is an economic division of labor by sex can infant dependency produce this effect (Dole, n. d.). In subhuman primates there is no sexual division of economic labor.

Social relations within the subhuman primate horde vary according to the age, sex and dominance statuses of the interacting animals. Leaving aside dominance for a moment, most social interaction can be adequately described by utilizing three elemental status categories: adult male, adult female, and young (Carpenter, 1942a: 180). Interaction of animals of these categories produce 6 "qualitatively distinct" social relations: male-male; female-female; male-female; male-young; female-young; young-young (Carpenter, 1940: 126). The content of most of these relations can be inferred from the preceeding and succeeding discussion. It will be seen that age and sex difference remain important social distinctions among primitive hunters and gatherers.

Dominance statuses are found among all known monkeys and apes, as well as many lower vertebrates (Carpenter, 1942a; Maslow, 1936a). Dominance is established by competition for mates, for food, for position in progression, and the like—". . . in every known typical grouping of monkeys and apes there is persisting competition for priority rights to incentives." (Carpenter, 1942a: 192). Conflict is often particularly prominent among males over females in oestrus. Males are usually dominant over females. [This last is subject to exception when females are in heat (Crawford, 1940; Nowlis, 1942; Yerkes, 1940). Dominance has been experimentally raised in primates and other vertebrates by injections of male sex hormones (Clark and Birch, 1945; Birch and Clark, 1946).] Dominance status affects behavior in every aspect of social life: play, feeding, sex, grooming, competition between groups, and it even determines the spatial relations of animals within the horde (Carpenter, 1942a; Chance and Mead, 1953; Maslow, 1936a; 1936b; Maslow and Flanzbaum, 1936; Nissen, 1951). Dominance among paired animals is easily determined experimentally by introducing a series of food pellets to which there is limited access, and noting which animal consistently appropriates them (Maslow, 1936a; Nissen, 1951: 447; Nowlis, 1941a; 1941b). Nowlis' food appropriation experiments, performed with differentially satiated animals, show that dominance is a social behavior arising from conflict—not a simple, independent drive for "prestige," as is sometimes claimed (Nowlis, 1941b; 1942).

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[7] This is a crucial physiological change for a number of reasons, not the least of which is that it makes intelligible how, when culture developed, primate sexuality was so radically subordinated to other ends.

Maslow contends that the quality of dominance varies among New and Old World monkeys and the apes, and that variation in dominance quality is correlated with differences in social organization (Maslow, 1940). Platyrrhines, according to Maslow, show the greatest indifference in social relations. Dominance is "tenuous," "non-contactual," frequently not expressed, and often ascertained in the laboratory only with difficulty. In contrast, "Catarrhine dominance is rough, brutal and aggressive; it is of the nature of a powerful, persistent, selfish urge that expresses itself in ferocious bullying, fighting and sexual aggression." (Maslow, 1940: 316). Weak and sick animals are attacked; in competition over food, a subordinate animal would starve. Chimpanzees however, show "friendly dominance." Dominant animals protect subordinates, never attack them except in the form of rough play. Crawford (1942) in one laboratory experiment noted that only 0.4% of chimpanzee social behavior could be described as aggressive, and other field and captivity data generally bear out Maslow's hypothesis of suborder differences in dominance quality (cf. Carpenter, 1934; 1937; 1942a; Collias and Southwick, 1952; Gillman, 1939: Harlow and Yudin, 1933; Maslow, 1936b; Warden and Galt, 1943; Yerkes and Yerkes, 1929; 1935; Zuckerman, 1932; for an exception, see Haddow, 1952).

There appear to be correlated differences in the grooming behavior of the suborders. Yerkes advances the notion that social, as opposed to self grooming, increases in frequency from prosimian through authropoid ape (Yerkes, 1933). Grooming serves a biological function in removing parasites and the like, but this is nearly equally accomplished by self or social grooming. Therefore, social grooming takes on added significance as a "social service" in the higher primates. Moreover, grooming in higher primates is not only social, but reciprocal. Social grooming among wild gibbons has been noted to be frequent and reciprocal. By contrast, evidence from field and laboratory indicates that social grooming is comparatively infrequent among New World monkeys. It is difficult to say from present evidence that social and reciprocal grooming increase from Old World monkeys to anthropoids, but on the whole, Yerkes assertions are supportable (cf. Carpenter, 1935; Crawford, 1942; Maslow and Flanzbaum, 1936; Warden and Galt, 1943).

The emergence of reciprocal social behavior and the progressive tempering of dominance relations are significant trends in the primate line—trends which, we shall note, are continued in primitive society. On the other hand, economic teamwork and mutual aid are nearly zero among subhuman primates, including anthropoids. Spontaneous cooperation—as opposed to one animal helping another—has not been observed among them. Chimpanzees have been trained to solve problems cooperatively, but fail to do so without tuition (Crawford, 1937). Monkeys apparently cannot even be taught to cooperate (Warden and Galt, 1943). Spontaneous teamwork presupposes symboling: "Teamwork makes intellectual demands of the same order as those made by language. Psychologically, it may, in fact, be difficult to distinguish between the two." (Hebb and Thompson, 1954: 540). Nissen and Crawford's elaborate experiment showed that chimpanzees share food pellets and tokens, although sharing was much less frequent than not, and was evidently non-reciprocal (Nissen and Crawford, 1936). However, in a similar experiment using animals tested for dominance, Nowlis observed that every case of sharing (in 480 trials there were 80 instances of sharing and, in these, one tenth of the food available was shared) was from subordinate to dominant; dominants never gave food to subordinates (Nowlis, 1941). Therefore, "food sharing" here is a function of previous dominance competition and actually indicates monopolization, not pooling, of a limited supply (Nissen, 1951: 447).

We now consider the social system of bands of hunters and gatherers. The usual band contains 20 to 50 people (Steward, 1955: 146), but during poor seasons it may fragment into small family groups. We have already looked at band structure. Corresponding to ecological conditions, bands range in composition from an enlarged patrilocal family to a congeries of variably related nuclear families. Almost all members will be kinsmen, and the etiquette of kinship behavior regulates social life.

In Australia, for example, kinship: "... regulates more or less definitely the behavior of an individual to every person with whom he has any social dealings whatsoever." (Radcliffe-Brown, 1930-1931: 43).

The family is the only social unit inside the band; where bands are unstable, it is the major form of social organization. Among primitives, the division of economic labor by sex is fundamental to the family and makes marriage an economic alliance—or, in Westermarck's terms, "... marriage is something more than a regulated sexual relation. It is an economic institution." (Westermarck, 1922, i: 26). The complementary economic roles of the sexes determine certain qualities of primitive marriage. First, it is a necessity for all adults; the unmated adult male of the subhuman primate horde rarely has a counterpart in primitive bands. Secondly, polygamy is usually economically impractical; monogamy is prevalent. Finally, as indicated, stable heterosexual relations are not simply determined by sexual attraction, but by economics. Sex is easily had in many hunting and gathering groups, both before and beside marriage, but such neither necessarily establishes the family nor destroys it. Legal rights to sexual privileges of spouses may be waived in favor of socio-economic advantages, as in wife lending. The very rules of exogamy and incest prevent the formation of the family on a basis of simple sexual attraction. Steward's statement of the economic basis of the Shoshoni marriage can be duplicated from accounts of other simple societies: "Marriage was an economic alliance in a very real sense . . . a union which brought into cooperation the complementary economic activities of the sexes—a person could not, in the interest of self-preservation, afford to remain long single . . . the role of exclusive sex privileges in matrimony seems to have been secondary." (Steward, 1938: 242). Compare Radcliffe-Brown on the Australians: "The family is based on the co-operation of man and wife, the former providing the flesh food and the latter the vegetable food . . . this economic aspect of the family is a most important one. . . . I believe that in the minds of the natives themselves this aspect of marriage, *i.e.*, its relation to subsistence, is of greatly more importance than the fact that man and wife are sexual partners." (Radcliffe-Brown, 1930-1931: 435).

In man, therefore, primate sexuality has been brought under cultural control; it has become, in part, a means to other ends. Another aspect of primitive marriage teaches the same lesson. Unlike primate unions, created and maintained in conflict, primitive marriage is a powerful factor in interfamilial and interband alliance. Again, as Steward writes of the Shoshoni, and one can find countless ethnographic paraphrases of this truism: "Marriage was more a contract between families than between individuals." (Steward, 1955: 118). Among hunters and gatherers, marriages are frequently arranged (or at least approved) by the families rather than the spouses. Considerations often pass between the groups, thus setting the pattern for future cooperation. Women may be exchanged between groups, or intermarriage between certain parties preferred and repeated, solidifying both the marriages and group relations. The alliance between families may be paramount to the extent that a marriage can survive the death of one of the partners, he or she being replaced through the levirate or sororate.

There is an outstanding implication of these characteristics of primitive marriage and the family. Given the division of labor by sex and the formation of domestic units through marriage, it follows that sharing food and other items, rather than being non-existent, as among monkeys and apes, is a *sine qua non* of the human condition. Food sharing is an outstanding functional criterion of man. In the domestic economy of the family there is constant reciprocity and pooling of resources. And, at the same time that kinship is extended throughout the band of families, so are the principles of the domestic economy. Among all hunters and gatherers there is a constant give and take of vital goods through hospitality and gift exchange. Everywhere, generosity is a great social virtue. Also general is the custom of pooling large game among the entire band, either as a matter of course, or in times of scarcity. Where kinship is extended beyond the local group by interband marriage, so are reciprocity and mutual aid. Goods may pass over great distances by a series of kinship transactions. Trade is thus established. Hunters and gatherers are able to take mutual advantage of the exploitation of distant environments, a phenomenon without parallel in the primate order. This emphasis on generosity, on mutual aid, and the attribution of social prestige for generosity, stand in direct opposition to the tendency among primates to monopolize vital goods. Perhaps the elaborated emphasis on sharing among primitives is to be partially understood as a cultural means for overcoming primate tendencies in the opposite direction.

In the system of social status, there is a generic continuity between primate and primitive society. Qualitative social differences of sex and age that are marked in subhuman primate groups are major principles of status and role allocation among hunters and gatherers. The division of labor by sex is an example. So is the pervasive recognition of sex, seniority and generation in kinship behavior and terminology (*e.g.*, see Radcliffe-Brown, 1948).

There is also some continuity in dominance status. Leadership falls to men among hunters and gatherers, although, what is possibly different from subhuman primates, it is especially the elders that are respected. There are, however, important qualitative differences in dominance relations among hunters and gatherers and subhuman primates. Each primitive band usually has an elderly headman. The respect accorded him and other elders is not due to their physical ability to appropriate a limited supply of desired objects. (They may be preferentially treated in communal food distribution, but this is another thing.) Quite the opposite from subhuman primates, a man must be generous to be respected. Prestige among hunters and gatherers can be estimated by noting who gives away the most—precisely the reverse of the test for dominance status among subhuman primates. The position of the head man rests primarily on superior knowledge of game movements, water and other resources, ritual and other things which govern social life. Thus Boas pointed out that there is a direct relation between the authority of Eskimo headmen of various groups and the distance and difficulties involved in traveling between winter and summer hunting grounds (Boas, 1888). But such knowledge alone cannot breed power. The leader of the band has no means to compel obedience. He is commonly described as ruling through "moral influence." A Congo pymgy leader bluntly told Schabesta, "There would be no point in his giving orders, as nobody would heed them." (Schabesta, 1933: 104). Steward comments that the title, "talker," given to a Shoshoni leader, "truly designates his most important function." (Steward, 1938: 247). The leader of the Central Eskimo camp is picturesquely referred to as *isumataq*, 'he who thinks' (for the others) (Birket-Smith, 1936: 148). In all bands of hunters and gatherers, the heads of the separate families exercise more control than the informal headmen over the whole. Compared to infrahuman primates, ranking hierarchies and dominance approach zero among hunters and gatherers. Yet, where all interact as kinsmen, and sharing a scanty food supply replaces conflict over it, this is expectable.

SUMMARY AND CONCLUSIONS

The transition from subhuman primate society to rudimentary cultural society was at the same time a process of generic continuity and of specific discontinuity. If culture had not developed among a species of primates, but among animals of different behavioral characteristics, then the forms and development of cultures would be basically different. The social behavior of primates is the foundation of some general features of human society. On the other hand, no specific trait of cultural society, even in its most rudimentary state is, in both form and functioning, a direct survival of some specific trait of primate social behavior. This discontinuity is due to the fact that subhuman primate society is a direct expression of the physiology of the species operating in a given environment; whereas cultural traits govern the social adaptation of the human primate. The development of culture did not simply give expression to man's primate nature, it replaced that nature as the direct determinant of social behavior, and in so doing, channeled it—at times repressed it completely. The most significant transformation effected by cultural society was the subordination of the search for mates—the primary determinant of subhuman primate sociability—to the search for food. In the process also, economic cooperation replaced competition, and kinship replaced conflict as the principal mechanism of organization.

What are the generic continuities? Territoriality is one. But similarities such as these are common to a wide variety of societies, including those of lower vertebrates. A more restricted continuity is the utilization of the powerful social functions of primate sexuality in human social organizations. To repeat an earlier observation, it is only against the unique background of primate sexual behavior that one fully understands why marriage and marriage rules are general mechanisms for integrating cooperative human groups. Another generic survival of simian society is the allocation of social functions on the lines of sex and age among hunters and gatherers.

Of particular interest are social advances within the primate order, upon which cultural society directly elaborated. Here the cultural developments appear capstones to trends which had begun to unfold in precultural conditions. In the primate line the exclusive mate group appears to have developed out of the promiscuous horde. In the transformation to the human family, the anthropoid mate group was altered more in function than in form. A second primate advance is the development of reciprocal social services in grooming. Generalized to food sharing, reciprocity is basic to cultural society. Thirdly, there is the softening of dominance relations among higher primates. In primitive society, dominance or prestige is especially associated with service to the group. (*See* also Hallowell, 1956, on generic similarities).

The most significant advances in the early evolution of cultural society can be deduced by comparison of primate and primitive groups. To us these advances appear to be: 1. the division of labor by sex and the establishment of the family on this basis; 2. the invention of kinship; 3. the incest prohibition and its extension through exogamy, thus extending kinship; 4. the overcoming of primitive competition over food in favor of sharing and cooperation; and 5. the abolition of other primate conflicts leading to the establishment of dominance hierarchies.

These 5 are complementary; nothing is said here of their order of appearance or relative significance. It is claimed that they are great triumphs of early culture. These developments are necessary social counterparts of the continuous tool activity which enabled man to become the dominant form of life.

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AGE CHANGES IN THE THICKNESS OF THE SCALP IN WHITE MALES

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INTRODUCTION

PURSUIT of two rather unrelated research problems has led to a number of investigations regarding the thickness of soft tissues covering the human cranium. First of all, physical anthropologists concerned with the comparability of measurements made on head and skull have amassed considerable data on scalp thickness at points involved in these measurements (*cf.* review by Stewart, '36). Secondly, interest in the question of balding has prompted studies of scalp thickness as a possible factor in this condition (Light '49, Garn, Selby and Young, '54).

Determinations of scalp tissue thickness have usually been made on cadavers. Parsons ('29), who employed lateral skull x-rays, appears to have been the first to study the living scalp. More recently, Light ('49) has published measurements of sections taken from the crown region during autopsies shortly after death.

The earlier work with dissecting room material established that scalp thickness tends to increase with age, and is generally greater in males than in females. Garn, Selby and Young ('54) confirmed these findings in living subjects.

The present paper describes changes in the thickness of the scalp along the arc between nasion and lambda in 20 white boys, viewed in x-rays taken between the ages of one month and 16 years. A sample of 50 men extends the study to adulthood. Fifty women were included so that possible sex differences in scalp tissue distribution could be investigated.

The sample was composed of white, native-born residents of southwestern Ohio, the majority of whom were of northwest European ancestry. Scalp thickness was measured on lateral skull x-rays at 20 points between nasion and lambda (figure 1) whenever possible, at 1, 3, 6, and 9 months, 1, 2, 3, 4, 6, 8, 10, 12, 14, and 16 years for each child. Comparable data were obtained from single x-rays of each of the 100 adults. All measurements were made at right angles to the outer scalp surface using vernier calipers calibrated to 0.1 mm.

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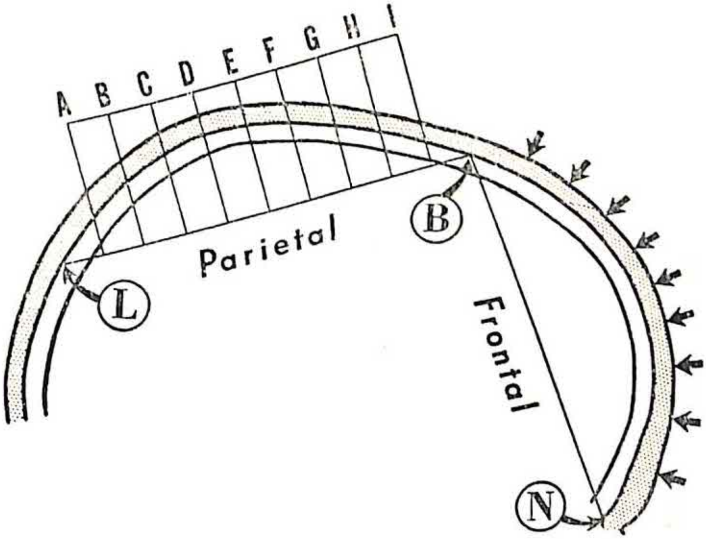

FIG. 1. DIAGRAM OF A LATERAL SKULL X-RAY; SCALP IS SHADED. N = NASION. SCALP THICKNESS WAS MEASURED AT LAMBDA (L) AND BREGMA (B). NINE ADDITIONAL POINTS OVER BOTH PARIETAL AND FRONTAL BONES WERE LOCATED BY ERECTING EQUALLY-SPACED PERPENDICULARS (A-I) FROM THE PARIETAL AND FRONTAL CHORDS, AS SHOWN. MEASUREMENTS WERE MADE WHERE PERPENDICULARS INTERSECTED THE OUTER SCALP SURFACE (ARROWS). CF. YOUNG ('56).

FINDINGS

As shown in table 1, there is an increase in thickness of the scalp along the mid-sagittal arc from one to about 9 months. From then until 3 or 4 years this dimension is stable, or may even decrease, following which steady thickening is again the rule until adulthood is attained.

Besides these general age changes, an alteration in thickness *pattern* is evident. Up to about 8 years the scalp is thickest in the glabella region. After one year, however, this advantage is progressively less, as scalp tissue absolutely diminishes on the lower forehead (figure 2). By 10 years, and thereafter, the scalp is thickest just anterior to lambda (somewhat behind vertex).

TABLE 1

Median scalp tissue thickness,[1] *in millimeters,*[2] *according to age*

			PARIETAL										FRONTAL									Average	
AGE	N	Lambda	A	B	C	D	E	F	G	H	I	Bregma	A	B	C	D	E	F	G	H	I	Median	Range
1 month	11–16	3.2	3.4	3.4	3.3	3.4	3.2	3.0	3.0	2.9	2.7	2.4	2.8	2.8	3.0	3.0	3.5	3.8	4.6	5.3	7.2	3.5	1.9–9.1
3	8–13	3.5	3.7	3.7	3.7	3.7	3.7	3.5	3.4	3.2	3.2	2.9	3.2	3.2	3.4	3.5	3.8	4.1	4.8	5.6	7.2	3.9	1.5–8.6
6	11–15	3.5	3.7	3.8	3.8	3.7	3.6	3.4	3.3	3.2	3.2	3.1	3.5	3.3	3.4	3.8	4.1	4.3	4.9	6.0	7.6	4.0	2.4–9.6
9	10–15	4.1	4.2	4.2	4.2	3.9	3.7	3.5	3.4	3.3	3.4	3.4	3.3	3.4	3.5	3.8	4.0	4.3	5.3	6.2	8.2	4.2	2.0–9.0
1 year	15–18	3.7	3.9	3.9	3.9	3.9	3.8	3.7	3.4	3.2	3.2	3.6	3.4	3.3	3.3	3.5	4.0	4.4	5.0	6.6	8.0	4.0	2.2–10.4
2	16–19	3.6	4.2	4.1	4.1	4.0	4.0	3.7	3.3	3.2	3.2	3.3	3.4	3.5	3.4	3.5	3.7	3.9	4.9	5.8	7.5	4.0	1.6–9.0
3	17–20	3.6	4.0	3.8	4.0	3.9	3.8	3.5	3.3	3.2	3.1	3.3	3.5	3.5	3.4	3.4	3.4	3.9	4.7	5.5	5.8	3.8	2.0–7.8
4	17–19	3.7	3.8	3.8	4.0	4.0	3.9	3.6	3.4	3.3	3.3	3.5	3.7	3.6	3.6	3.4	3.3	3.7	4.3	5.1	5.6	3.8	2.6–7.5
6	18–20	3.8	4.0	4.3	4.3	4.1	4.0	3.8	3.6	3.6	3.7	3.5	3.8	3.8	3.8	3.5	3.3	3.6	4.0	4.6	5.4	3.9	2.5–6.1
8	17–18	4.0	4.7	4.6	4.5	4.5	4.2	4.2	3.8	3.8	3.7	3.7	3.9	3.9	3.9	3.5	3.4	3.5	4.2	4.8	5.4	4.1	2.6–7.0
10	19–20	4.4	4.5	4.3	4.7	5.0	4.7	4.4	4.2	4.1	4.2	4.1	4.5	4.3	4.2	3.9	3.7	3.7	4.2	4.7	5.5	4.4	2.7–7.3
12	20	4.9	5.6	5.7	5.5	5.2	5.1	4.7	4.5	4.6	4.6	4.9	4.9	5.0	4.6	4.5	4.0	4.1	4.3	4.9	5.6	4.9	2.8–7.5
14	19–20	5.5	6.1	6.1	6.3	6.2	5.5	5.4	5.1	5.1	5.1	5.0	5.5	5.5	5.0	4.5	4.2	4.2	4.4	5.3	5.8	5.3	2.9–8.4
16	12	6.2	7.1	6.8	7.0	6.5	6.0	5.7	5.6	5.6	5.7	5.6	6.0	6.0	5.7	5.5	5.0	4.6	5.0	5.5	6.1	5.9	3.4–8.8
Adult	♂48–50	7.0	7.4	7.2	7.0	6.6	6.2	6.0	5.8	5.7	5.7	5.6	5.9	6.0	6.0	5.7	5.0	4.7	4.7	5.7	5.7	6.0	3.2–13.0
Adult	♀49–50	6.4	6.9	6.8	6.8	6.4	5.9	5.7	5.4	5.4	5.4	5.5	5.6	5.6	5.3	5.0	4.3	4.1	4.5	5.1	5.7	5.6	2.6–10.5

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[1] Reading from left to right (i. e., beginning with lambda), the medians refer to successively more anterior regions in the mid-sagittal plane (cf. figure 1).

[2] Uncorrected for x-ray enlargement (4.2–5.0%).

Similarly, the thinnest part of the scalp before 4 years is in the bregma region (slightly in front of vertex). After 4 years the scalp is thinnest on the upper forehead.

Examination of individual curves completely substantiates the conclusions drawn from group medians.

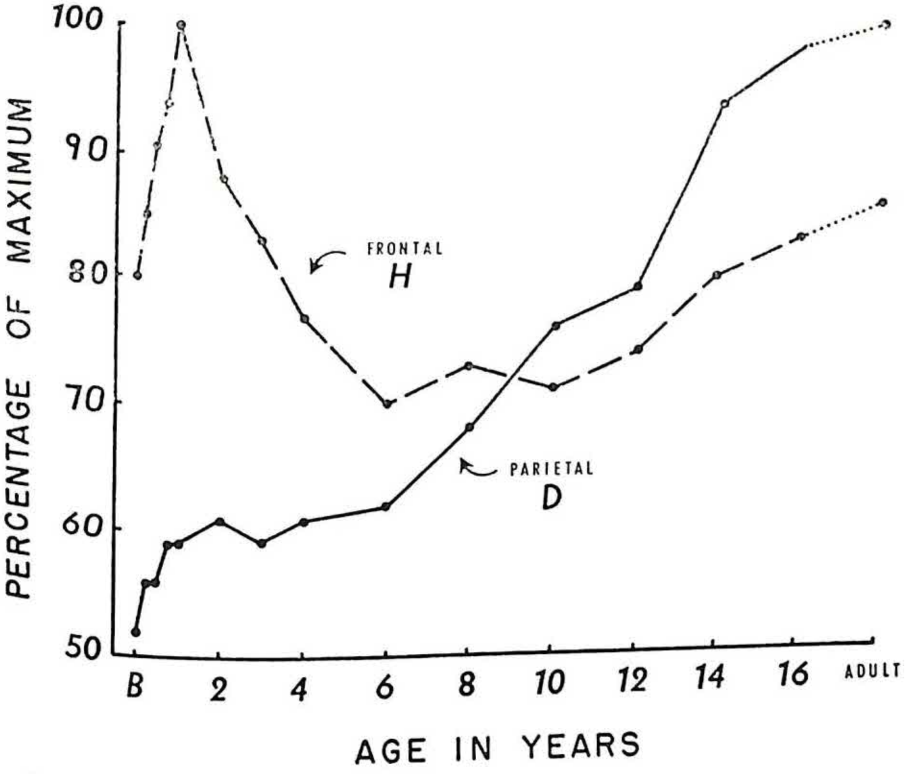

FIG. 2. PERCENTAGE OF MAXIMUM SCALP THICKNESS, BY AGE, AT TWO OF THE 20 POINTS MEASURED. NOTE THAT SCALP TISSUE IS LOST OVER THE LOWER FOREHEAD (FRONTAL H) AT THE SAME TIME IT IS GAINED NEAR THE VERTEX (PARIETAL D).

As reported in earlier studies, adult male scalps are thicker than their female counterparts. The current sample reveals no sex differences in distribution of adult scalp tissue along the mid-sagittal arc.

DISCUSSION

In general, the scalp thickens with age. Between 9 months and 3 or 4 years, however, it changes very little or may even decrease in thickness in some regions. A definite thinning, in fact, occurs over the lower forehead during this period, and continues until the age of about 8 years. These findings are consistent with previous reports which have demonstrated a loss of subcutaneous tissue at various body sites between the age of about 9 months and 6 years (Garn, Greaney and Young '56, Kornfeld '57; earlier references in Tanner '55).

It was observed that during early childhood the scalp becomes thinner over the forehead at the same time it increases in thickness near the vertex. Similar alterations in the pattern of subcutaneous tissue thickness have been noted in older age groups: Meredith ('35) recorded that in males from 12 to 18 years subcutaneous fat decreases in thickness on the arm while increasing on the thorax. Concurrent fat loss and fat gain in adults was documented by Garn and Young ('56), who showed that anterior leg fat may diminish while trochanteric fat increases.

It is unlikely that any single factor will eventually account for all these phenomena. Taken together, however, they show that soft tissues may *decrease* in thickness in certain parts of the body as a normal component of physical growth and development. Moreover, these tissues may simultaneously *increase* and *decrease* in amount at different sites. "Redistribution" of subcutaneous tissues, a well-known characteristic of pubescence (Tanner '55) may ultimately be found to be a continuing process throughout the life span.

With regard to the scalp, the situation is complicated by the fact that the single measurement of "scalp thickness" includes five histologically distinct layers: skin, fat, muscle (or aponeurosis), loose connective tissue and periosteum (Learmonth '51). Most reasonably, the fat layer (which constitutes roughly 50% of total thickness in adults) (Light, '49) would be expected to contribute most to thinning of the scalp over the brow during early childhood. Unfortunately, histological material is not currently available for the period in question.

It is noteworthy that the scalp is thickest in infants where the underlying bone is also particularly thick (Drey '57); that is, in the supraorbital region.

Finally, it may be pointed out that changes in scalp tissue thickness during childhood may account for some of the irregularities in growth curves reported in anthropometric studies of the head (*e. g.*, Davenport '40).

SUMMARY

1. Scalp thickness was measured at 20 points between nasion and lambda on serial lateral skull x-rays of 20 boys who had participated in the Fels Research Institute study from one month to 16 years of age. Similar measurements were made on x-rays of 100 adults.

2 Scalp thickness increases from one to 9 months, is constant or decreases from 9 months to 3 or 4 years (or later in certain regions), then increases until adulthood.

3. Up to age 8 the scalp is thickest at glabella; thereafter it is thickest just in front of lambda.

4. Before 4 years the thinnest part of the scalp is in the area of bregma. After that age the thinnest part is on the upper forehead.

5. These findings are consistent with the results of studies which have described changes in the distribution of soft tissues in other regions of the body at various intervals during the life span.

6. Although male scalps are thicker, there is no apparent sex difference in the pattern of mid-sagittal scalp thickness in adults.

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OSTEOLOGICAL CONSEQUENCES OF SCALPING

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INTRODUCTION

SCALPING has long been recognized as a custom which held great cultural significance for those performing the scalping operation and critical or acute significance for those persons being scalped. In the central and eastern United States, where scalping was a prominent feature of warfare between various American Indian tribes, there have been a number of studies dealing primarily with the cultural aspects (Friederici, '07). The possible consequences of scalping manifested in the victim, however, have been relatively ignored (Reese, '40). Aside from the obvious paucity of materials the delay in investigating such consequences has perhaps been most closely related to two common assumptions concerning the practice. First, the evidence of scalping was transitory in that only soft part morphology was affected, i. e., the scalp alone was removed and preserved as a trophy. Second, the victim was killed or mortally wounded prior to the scalping or died soon after and therefore no characteristic secondary changes in the cranial vault were to be seen in the osseous remains. While generally true there have been sufficient exceptions to provide material for study which may accomplish the twin objectives of an understanding of the osteological consequences of scalp removal, and of eventually determining the historical depth of the practice. It is likely that significant specimens are now lying unnoticed in laboratory collections. The number of cases available for study will always remain small in view of the fact that most victims died immediately before or after the scalping, thus obviating any secondary alterations in the vault such as exfoliation of the outer table and replacement with new bone. Nevertheless, scalping like trephining, offers a unique contribution to paleopathology for it is only in these two practices that the scalp was systematically removed or reflected. This set in motion the physiological consequences attendant upon deprivation of normal blood supply to the surface of the cranial vault.

HISTORICAL REVIEW

There are few papers in the literature attempting identification of scalping (Neumann, '40; Snow, '41). The cases described are limited to noting cut marks, primarily on the frontal, parietal and occipital bones. While clearly caused by knives, alternatively axes or celts, there is no evidence that the victims survived the scalping and thus no secondary changes. One possible case of scalping belongs to the Middle Mississippi culture (ca. 1000-1500 A. D.) of Central Illinois (Neumann, '40). Aside from this case there is little evidence for the antiquity of the practice in the United States. There is ample evidence for the taking of the entire head as seen in the finding of numerous "trophy skulls" in the earlier Hopewell culture (Willoughby, '22). It has been suggested that scalping represents a later practice, possibly derived from the earlier practice of taking the entire head.

Ethnological evidence clearly indicates that Southeastern United States Indians, as well as the Chichimecs of Mexico and the Huron of the St. Lawrence River, were taking scalps in the early sixteenth century. Thus, the Muskhogean-speaking tribes of the Southeast were practicing scalping when visited by DeSoto in 1540. The great range of variation in size of scalp removed, method of removal, treatment of the scalp, etc., are abundantly documented in the literature (Friederici, '07; Knowles, '40; Reese, '40).

SOURCE OF MATERIAL

During the summer of 1956 an archaeological party from the University of Wisconsin, under the supervision of Professor D. A. Baerreis, recovered two series of skeletons, tentatively assigned to the Arikara Indians, in village sites near Mobridge, South Dakota. This research was sponsored by the University of Wisconsin, and by contract with the National Park Service, to salvage archaeological materials in the Missouri River basin that were to be flooded by construction of the Oahe Dam. These villages appear to belong to a period between 1690 and 1720 A. D.

A series of skeletons from the Spiry-Eklo site (39 WW 3), consisting of three adult females, one adult male, three children and some 19 infants, presents many interesting biological and cultural problems. Traumatic behavior is not limited to the single case under discussion. Also present was a decapitated female represented solely by the skull, two cervical vertebrae and the hyoid bone. The base of the cranium, the mandible and the vertebrae bear many knife or axe marks indicating clearly the method of severing the head from the body.

DESCRIPTION OF SPECIMEN

Of primary interest here is a skeleton with an unusual lesion on the skull. This skeleton (U. W. Laboratory of Physical Anthropology No. 2/7) is that of a small but rugged and masculine female, 50–60 years of age. Only the second right molar of the maxillary dentition remains in place, loss of the other maxillary teeth has contributed to collapse of the maxillary sinuses and thus to enlarged sub-orbital fossae. All crowns of the mandibular teeth have been severely abraded or are completely worn away. The left wing of the sacrum is larger than the right, and at least four lumbar vertebrae have been partially compressed. Exostoses are present in several places such as the borders of the thoracic and lumbar vertebrae, the distal end of the right femur and the innominate bones. In form the cranium is low and is mesocranic with a cranial index of 77.5; the nasal bones are high and prominent. In all respects, metrical and non-metrical, this skull presents a usual combination of traits found in the other Arikara crania.

The surface of the skull in general is smooth, and while the course of the sutures can be discerned they are in many places obliterated endocranially. At the back of the skull an elliptical area of 7 to 10 cm in the longest diameters is easily distinguished (fig. 1). Its surface is smooth but very uneven and shows many flattened depressions (fig. 2). Only small remnants of the sutures remain in this affected area (fig. 1).

For intensive examination we employed a method which had proved very useful to one of us in the examination of old Peruvian skulls (Hamperl and Weiss, '55). This consisted of cutting a thin 2 mm section with a saw and placing it on an X-ray film to make a 'hard' picture. The inner structure of the bone is consequently much more visible than on the cut surface.

On such a section the normal thickness of the skull is 4–5 mm, whereas in the affected area it amounts to only 2–3 mm (fig. 3). Alteration of the bony structure can easily be seen by the naked eye. On the X-ray film (fig. 4), the inner table is seen to be minimally delimited, forming a continuous line from the normal bone through the involved area. The outer table is similarly present, but somewhat thicker in the normal bone at either side of the affected area than within the affected area. Most striking is the variation in structure of the diploe: in the normal areas at the edge of the section (fig. 4) the diploe shows the usual coarse, reticular structure, whereas in the central affected area the reticules are much smaller and sometimes so dense that

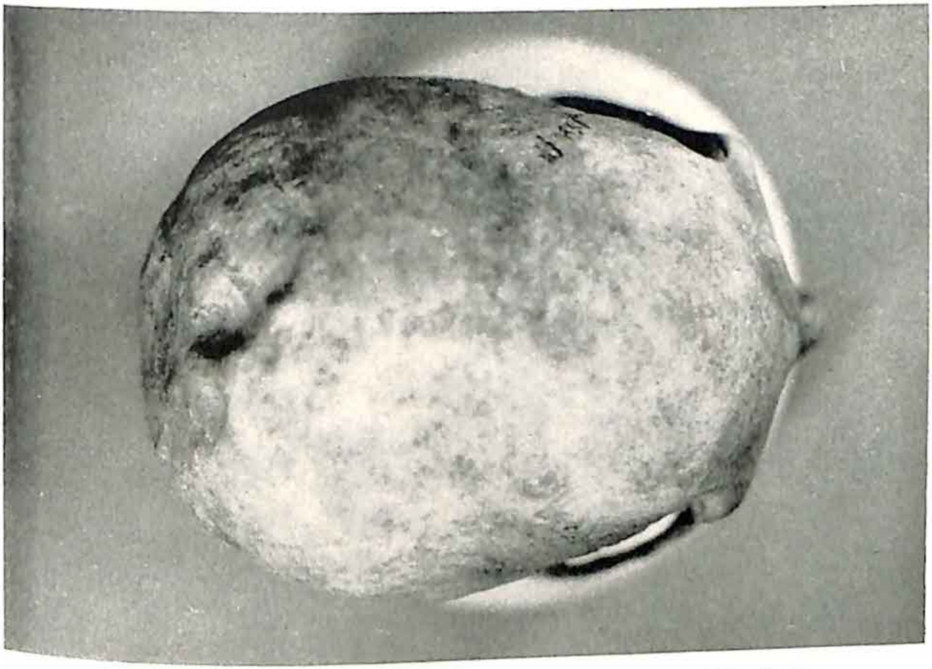

FIG. 1. DEPRESSIONS IN THE OCCIPITAL REGION OF THE SKULL.

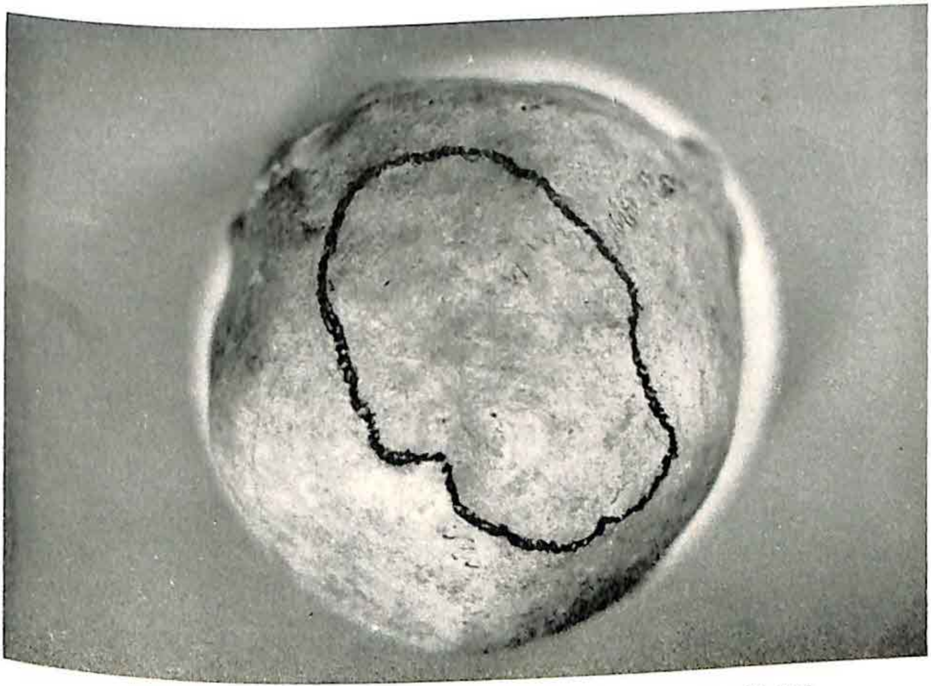

FIG. 2. EXTENT OF THE INVOLVED AREA OUTLINED IN BLACK.

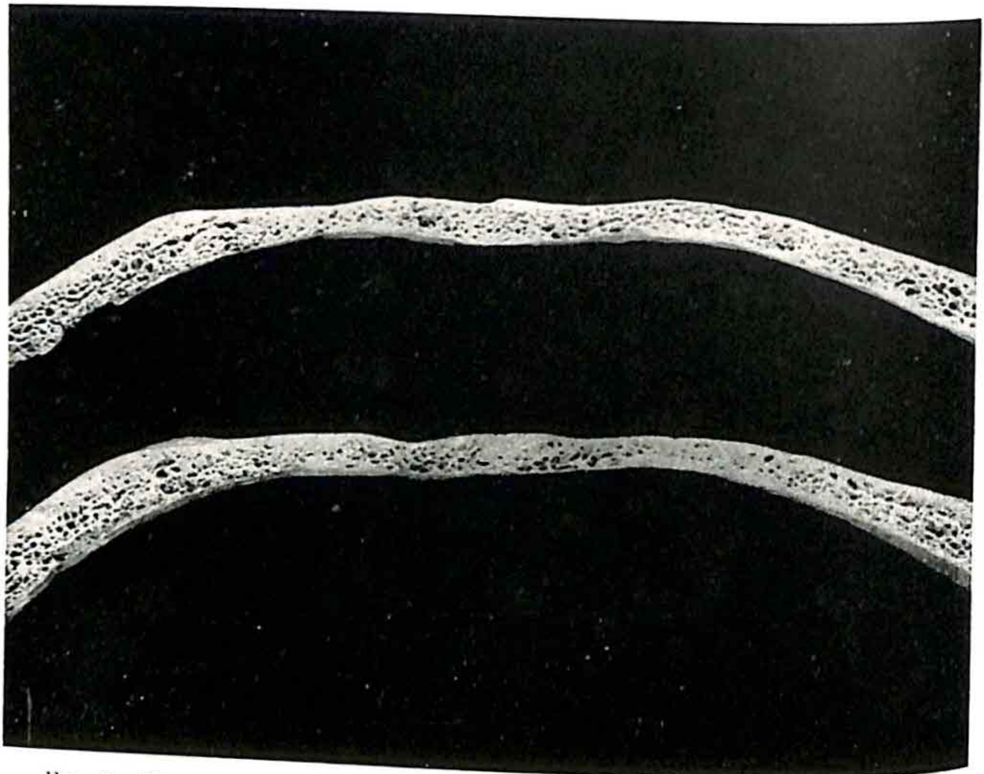

FIG. 3. TWO SECTIONS SHOWING THE THINNER, ALTERED BONE IN THE INVOLVED AREA AND THE THICKER BONE OF THE NORMAL AREA ON EITHER SIDE WITH THE COARSER STRUCTURE OF THE DIPLOE. (SLIGHTLY ENLARGED)

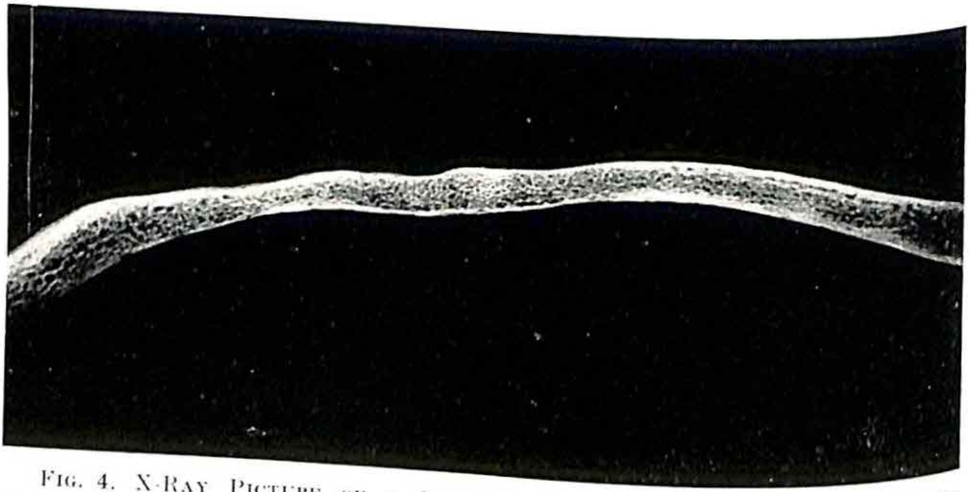

FIG. 4. X-RAY PICTURE OF A 2 MM SECTION SHOWING THE CONTRAST IN STRUCTURE OF THE DIPLOE BETWEEN THE CENTRAL AFFECTED AREA AND THE NORMAL AREAS AT EITHER SIDE.

an almost compact bone results. Only in areas immediately over the inner tables can some coarse meshes of the normal diploe be recognized.

DISCUSSION

In attempting to explain these findings we must distinguish precisely between what may be termed formal genesis and causal genesis. Concerning the former we are able to make some well founded statements based on our present knowledge of the pathology of bone; concerning the latter we are bound to express only more or less tentative conjectures invoking circumstantial evidence.

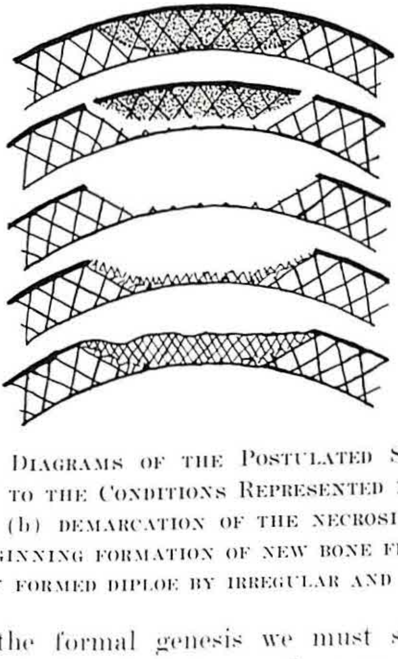

FIG. 5. SCHEMATIC DIAGRAMS OF THE POSTULATED SEQUENCE OF CHANGES WHICH MAY HAVE LED TO THE CONDITIONS REPRESENTED IN FIGS. 3 AND 4. (a) SUPERFICIAL NECROSIS, (b) DEMARCATION OF THE NECROSIS, (c) EXFOLIATION OF NECROTIC BONE, (d) BEGINNING FORMATION OF NEW BONE FROM REMAINING DIPLOE, (e) COVERING OF NEWLY FORMED DIPLOE BY IRREGULAR AND THIN EXTERNAL TABLE.

In interpreting the formal genesis we must start from one well established fact, *i.e.*, in the affected area the normal bone has entirely disappeared with the exception of the inner table and possibly some contiguous portions of the diploe, and has been imperfectly replaced by new bone of different structure. This replacement could have taken place either in the form of a slow continuous transformation of the old bone by resorption and new formation of bone matrix, or alternatively, as an outgrowth of new bone after a rather sudden loss of the old bone. The sharp limits of the affected area speak against the assumption of a slow transformation of the preexisting bone, as such a process usually involves wide areas without sharp borders. Similarly, it would be difficult to explain the thinning of the bone as these transformations lead rather to a thickening of bone. Therefore, a rather sudden loss of normal bone seems to us more probable. The ensuing defect obviously extended from the outer surface to the inner table and was successively refilled by new bone which no longer retained the structure of the lost bone. In other words, we assume that the missing part of the old skull vault was exfoliated through a defect of the skin.

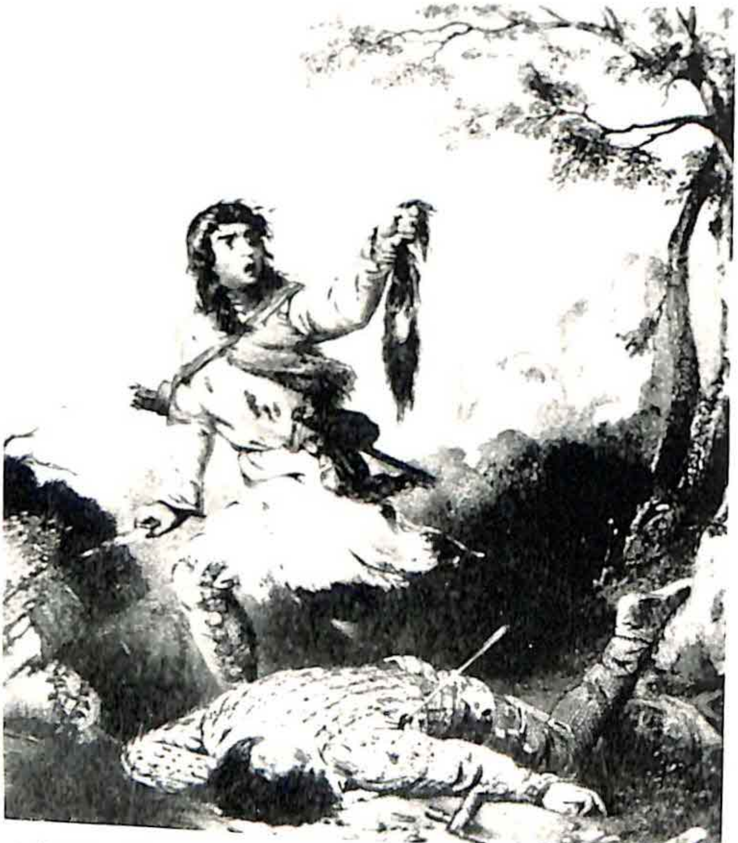

FIG. 6. "DEATH WHOOP OR CRY," FROM EASTMAN'S ABORIGINAL PORTFOLIO. (REESE, '40) THE SCALPING WOUND CORRESPONDS IN SITE AND SIZE TO THE BONE LESION OF THE CASE PRESENTED.

The sequence of events, as we have tried to reconstruct them on the basis of the anatomical findings is schematically depicted in fig. 5. In summary, we should like to interpret the lesion as a form of scar tissue arising from a superficial injury to the bone.

There remains the question of possible causes of such a defect. One may consider some bacterial or traumatic influence. An inflammation caused by micro-organisms can lead to more or less extensive necrosis of bone either by toxic influence or by impairment of the blood supply to a given area. The necrotic parts are consequently separated from the living bone by granulation tissue and exfoliated, a process called demarcation and sequestration. Two organisms are especially capable of producing such lesions on the skull: the staphylococcus and the spirochaeta pallida. The staphylococcus causes a purulent osteitis and periosteitis almost exclusively confined to the long bones and only in exceptional cases does it affect the bones of the skull. In such a case one would expect rather a marked outgrowth of bone, not a thinning as in our case. For these reasons we may dismisss the possibility of a staphylococcus infection as quite improbable. In contrast to the staphylococcus the spirochaeta pallida more frequently affects the skull, but the site of preference is the frontal bone. Furthermore, one would also expect a more distinct formation of new bone in excess of the lost bone. For these reasons a microbial infection seems to us very improbable as the cause of this lesion.

The remaining possibility is traumatism. One can easily imagine a tomahawk blow cutting off not only the skin but part of the bone and thus producing the wound whose healing resulted in the bone lesion found here. It would, however, be a matter of chance if a blow were inflicted in just the tangential direction necessary to cut off such a piece of bone and skin together. We know from experience that tangential blows usually become wedged in the vault and only break off a small wedged-shaped piece of it. Therefore, even such a trauma as this appears to us an unlikely explanation of the lesion.

In the case under consideration there is one other possibility for which traumatism may be considered, *i. e.*, a non-lethal scalping which healed in the surviving victim. Such an assumption is contradicted neither by the site and size of the bone lesion nor by the sex of the individual. As is well known, scalpers did not always remove the entire scalp above the ears but often took only a top-knot or comparatively small patch from the occipital, lambdoid or vertex region. A contemporary picture of scalping (fig. 6) shows removal of a scalp at exactly the same site and of the same size as that of the bone lesion found in our case. It should furthermore be remembered that scalping was not confined to males but extended to females, both young and old.

One might be inclined to dismiss the possibility of a healed scalping wound as highly improbable were it not for a report in the literature where the healing of such a wound is described. The actual events correspond exactly to our conjecture based on the formal analysis of the skull in our case. The following description is given by the attending physician, Dr. R. C. Moore (Reese, '40, pp. 18-19)

> Wm. Thompson, an employe of the Union Pacific Rail Road Company, was scalped by the Cheyennes near Plum Creek Station, Neb. . . . Aug. 6, 1867. He was placed under my care . . . 36 hours after the wounds were inflicted.
>
> The scalp was entirely removed. . . . The pericranium was in places detached, but the greater portion . . . was dried and adherent to the bone. . . . The only dressing used during the whole course of treatment was surgeons' lint, saturated with pure olive oil, which excluded the air and was easily removed. . . . Healthy granulations soon appeared on the tissue surrounding the denuded calvarium, but showed no disposition to extend over the bone. In about three weeks the outer table began to exfoliate, first at the margins, then under the adherent pericranium. . . . Granulations sprung from the diploe, till the entire surface presented the appearance of a healthy wound. The last portion of the outer table to become detached were the spots from which the pericranium had been removed.
>
> The suppuration was profuse but the patient, being strong . . ., did not . . . present those symptoms of depression . . ., nor were there any symptoms indicating that the inflammation had extended to the brain or its membranes. . . . In about 3 months from the time the scalp was removed, nearly the entire surface was cicatrized.

The pathologist would add to this very concise description, that the periosteum of the skull bones was either removed by the process of scalping or what was left on the surface of the bone soon dried to shreds. In both instances the bone was deprived of the normal blood supply through the outer periosteum so that superficial ischemic necrosis became inevitable. The necrotic bone was then separated from the living bone by granulation tissue originating in the depths of the diploe and was eventually exfoliated as a whole or in pieces. We have good reason to presume that the endosteum formed new bone to fill the defect until the whole wound was covered by epidermis. This appears the most likely etiology of the lesion in our skull.

In discussing the causes of osteitis or of scarring around surgical openings in the bone of trephined Peruvian skulls, Stewart has invoked the same kind of explanation (Stewart, '56, pp. 303-304).

Particular attention is called to case No. 293, 778 in table 2 in which there are two openings, one fresh-looking and surrounded by signs of osteitis (in an angular pattern), and the other completely healed, but surrounded by an angular area of scarring. This finding suggests that the scarring defines the area that had the outer table exfoliated by osteitis and subsequently healed over.

SUMMARY

The osteological consequences following scalping, seen in the skull of a protohistoric Arikara Indian who survived the experience, are described. These include exfoliation of the outer table and diploe, and subsequent replacement with new bone. The similarity of the resulting osteitis to that accompanying trephination, where the scalp was reflected as a preliminary to the operation, appears patent.

Though the number of such cases will necessarily remain small, even a few well studied examples will yield pertinent data for paleopathological investigations. At the same time such examples will provide indications of time depth and geographical distribution essential to understanding the history of this practice.

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GROWTH OF THE MAIN HEAD DIMENSIONS FROM BIRTH UP TO TWENTY YEARS OF AGE IN CZECHS *

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INTRODUCTION

CONSIDERABLE attention has been given recently to the systematic study of growth and physical development of children and adolescents, both in Czechoslovakia and elsewhere. Substantially accelerated growth has occurred in recent years in nearly all of the civilized nations of the world, and it is to be ascribed chiefly to improved nutrition and other social and sanitary measures which have especially promoted the welfare of the young.

As reviewed by Dokládal ('55b), scores of publications have appeared in the past decade on the stature, weight and chest circumference of children in Czechoslovakia, but hardly any further data are yet available on the trunk and limbs. The head has been especially neglected, and where data are available at all, few children have been measured or only one or two age categories investigated.

This deficiency of information on local samples has made it necessary for most writers on pediatrics, hygiene and somatology in Czechoslovakia to use data on the growth of the head from Germany, the Soviet Union, or North America, without accurate knowledge of whether or not these statistics actually applied to the Czechoslovak population. Recent publications from the pediatric clinic in Hrádec Kralové indicated that the application of foreign data as norms for Czech children is highly erroneous (Blecha *et al.*, '53, '54).

Especially for pediatricians and school physicians, the dimensions of the head—notably its circumference—are considered along with other physical and perhaps also mental features as criteria for assessing the general development of the child. Although head circumference is not appreciably correlated with the intelligence quotient, it still possesses some diagnostic value to the clinician (Boyd, '45).

In the present paper we have obtained data concerning the growth

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of the head among Czech children in Brno from birth to 20 years of age. The objectives of this work have been to add to our knowledge of physical growth in the Czechs, and to investigate the ontogenesis of brachycephaly and dolichocephaly. The age changes in the cephalic index will be dealt with in another paper.

A BRIEF REVIEW OF THE PREVIOUS LITERATURE

Řehák ('23) measured the circumference, length, breadth and height of the head in 1488 Czech boys in Prague and Nový Bydžov. The age range was 6–18 years, and about 100 boys were measured at each age. Unfortunately, his investigation did not include girls.

Štampach ('30) undertook a similar study of Slovak children in the area of Trenčín. Although some data on girls are included, his series is comparatively small, and exceeds 30 individuals in only a few age categories.

Other data of Czech authors are fragmentary. Kubíček ('48, '50) gives norms of head circumferences at birth, 3, 6, 9 and 12 months of age. He also gives such data for children at 3, 5, 7, 10, and 12–15 years, but does not separate the sexes nor indicate the sizes of his normative samples. Kubíček regards these data as provisional.

Rejlek ('50) reported on the head circumference of 403 boys and girls at 3–3½ years of age, and Došková ('23) published statistics on the length, breadth, height and circumference of the head of a small series of adolescent boys, with about 25 individuals in each age category.

Somewhat more extensive work was done in Brno among secondary-school students. As part of a more general program of somatic measurements, the length and breadth of the head were included. Ptáček ('51) studied 804 individuals from 12 to 15 years old. Votava ('52) investigated about 1000 individuals of both sexes, ranging from 16 to 20 years of age. Their data are still unpublished and practically unknown.

Further information on the dimensions and growth of the head can be found in anthropometric studies of adults in Czechoslovakia. Dokládal ('53) studied age changes in the length and breadth of the head and in a number of facial dimensions among the inhabitants of the district of Hlučín in Moravian Silesia. This series comprised 147 men and 117 women, ranging in age from 18 to 70 years. Prokopec ('57) obtained data on the head circumference of 1,100 forest laborers from all over Czechoslovakia, ranging from 16 to 75 years of age. Suchý ('56) reported on the head circumference of children and adults from the area of Brumov. His series comprised 39 boys and 48 girls from 7 to 12 years of age, and 100 men from 16 to 59 years old.

Some information is also available on the head circumference of infants and young children. Blecha *et al.* ('53, '54) investigated 1600 infants during the first postnatal year, and report data for both sexes at each month of age. Šobova ('57) reported on the growth of this measurement from birth to three yeares of age in 4423 individuals from Prague and in 2590 from the district of Pardubice.

Although the world literature on the dimensional growth of the head is far more abundant than the local publications from Czechoslovakia, data are few on the period after World War II, when secular changes in at least some measurements of growing children were probably conspicuous in some populations.

Among the more extensive studies of head circumference in European children are the works of Daffner ('02) and Pfaundler ('16) in Germany. In Belgium during the 19th century, Quetelet (1871) presented measurements of both sexes from birth to 20 years of age. In Russia, Bondyrev measured both sexes from one to 15 years of age at about the turn of this century (*cf.* Matiegka, '27). Cwirko-Godycki ('26) and Czaplewski *et al.* ('33, '34) measured series of Polish boys in Poznan. Children aged 7–13 years from Kashub, near the Baltic, were measured by Cwirko-Godycki and Wrzosek ('37). Matiegka and Chrapko ('40) studied young Ruthenians from 6–14 years of age.

Noteworthy studies of head circumference in infancy are those of Brock ('32), Nobécourt and Babonneix ('34), Bayley and Davis ('35), Orlov ('51), and others.

European studies of the length and breadth of the head in childhood include a study of Germans from birth to 15 years by Ranke ('05), including both sexes. Roese ('26 [*cf.* Martin, '28]) measured series of Germans of both sexes aged 10–17 years. Vyazemsky produced similar statistics on Russian boys aged 10–18 years (*cf.* Matiegka, '27). Schwerz ('11) presented data on Swiss boys 6–20 years old, and girls 6–16. Godin ('22) published these measurements on French adolescents of both sexes, aged 13–17 years. Polish data from Poznan are also in the literature (Cwirko-Godycki, '26; Czaplewski *et al.*, '33, '34). In Cracow, Jasicki ('38) presents data on boys 6 to 18 years of age, and statistics for Kashub may be found in Cwirko-Godycki and Wrzosek ('37). In the United States, Bean ('34) published data on both sexes from 8 to 20 years old, and Goldstein ('36) on boys from 2 to 21. For head breadth only, an exhaustive review of the literature on the first 12 years of life was published by Meredith ('53).

Although the world literature on the growth of the head is by no means scarce, no single study is wholly satisfactory. Usually the age range has been too restricted, only one measurement has been reported, or the study has been limited to one sex. Up to the present, no single investigation of length, breadth and circumference of the head has covered the whole postnatal period of growth in both sexes. Data sometimes differ considerably for reasons which may have resulted from variations in technique, which is not surprising if we take into account the great distances in both time and space between separate investigations. As a result, tabular data on the first 20 years of life such as those of Berry and Porteus ('20) have often relied only in part on direct measurements, while the rest of the material was obtained by mere mathematical calculations.

METHODOLOGY OF THE PRESENT STUDY

A cross-sectional investigation was undertaken of 5,674 subjects, including 2,896 boys and 2,778 girls. The age range was from birth to 20 years, and for each sex, the average number of children measured at each age was about 100. The neonatal series was measured from April to June of 1954, and the remainder in January and February, 1955. All the subjects are of Czech nationality and resident in Brno. The neonatal sample was measured in the Clinic of Newborns of the Maternity House in Brno. Those from 6 months to 3 years of age came from homes for infants and children. The group from 3 to 6 years of age came from kindergartens. The older school groups were obtained from the various educational institutions. The subadult sample from 15 to 20 years of age in part was taken from unpublished data collected by Dr. L. Votava, with his kind consent.

My thanks are due to the health and educational authorities of the Brno Town Council, to the Director of the Municipal Institute of Public Health, and to the professional staffs of all the institutions concerned.

The selection of institutions for the present research was made purely at random in order to include all social categories: children of professional people, workers, and farmers alike.

Three measurements of the head were taken in conformity with the technique of Martin ('28): the length from glabella to opisthocranion, the maximal breadth, and the horizontal (fronto-occipital) circumference. The diameters were taken with a metal caliper, and the circumference with a measuring tape. A new tape was used for each 200–300 individuals because the old one was liable to stretch 1 mm or slightly more with excessive use. Further details on my techniques of measuring the newborn are published elsewhere (Dokládal, '55a).

From birth through the pre-school ages, only healthy children were selected—especially those without any defective growth or development of the head. Of the total number of 5,674 subjects, 32 were not included in the statistics for various reasons such as foreign nationality or lack of personal data. Thus, after these exclusions, the series numbered 2,879 boys and 2,763 girls. During the anthropometry of a particular class or group of children, subjects whose dimensions seemed improbable were remeasured.

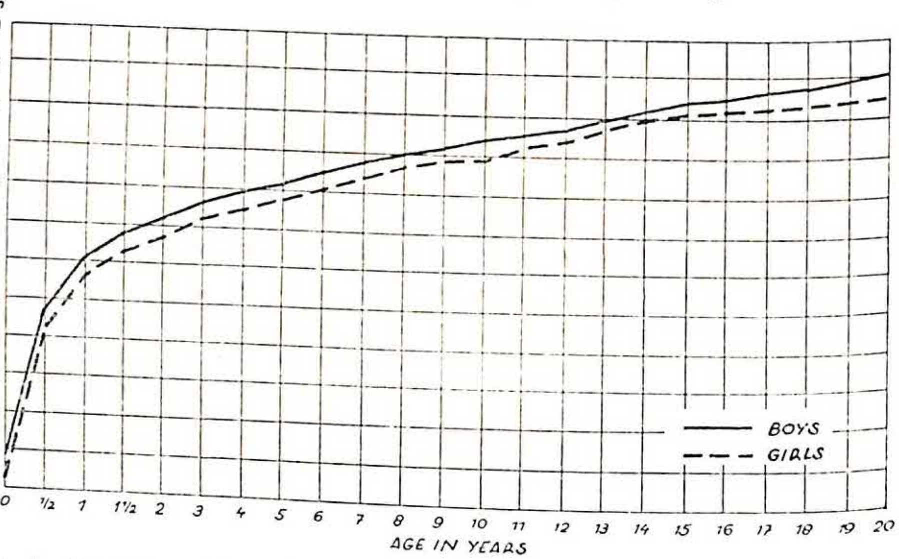

FIG. 1. GROWTH OF HEAD CIRCUMFERENCE FROM BIRTH TO 20 YEARS OF LIFE.

The ascertainment of precise age for each individual is exceedingly important. At two years and older, age was defined in terms of the nearest birthday. The 6, 12 and 18 months categories consisted of infants within three months of these ages. Hence, the age label is the midpoint of the range measured in each category.

For each sex separately, the three measurements are subgrouped into 23 age categories, and each category analyzed statistically for central tendency and variability (tables 1–6). Columns of increments per year and of the percentages of the mature measurement have also been included, and the average trends graphed (graphs 1–3).

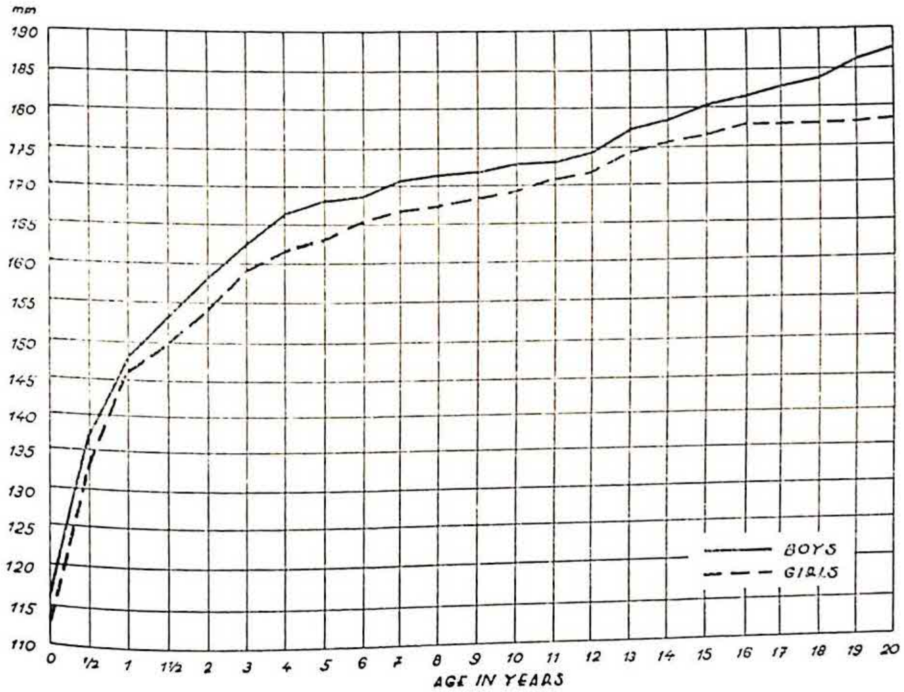

FIG. 2. GROWTH OF HEAD LENGTH FROM BIRTH TO 20 YEARS OF LIFE.

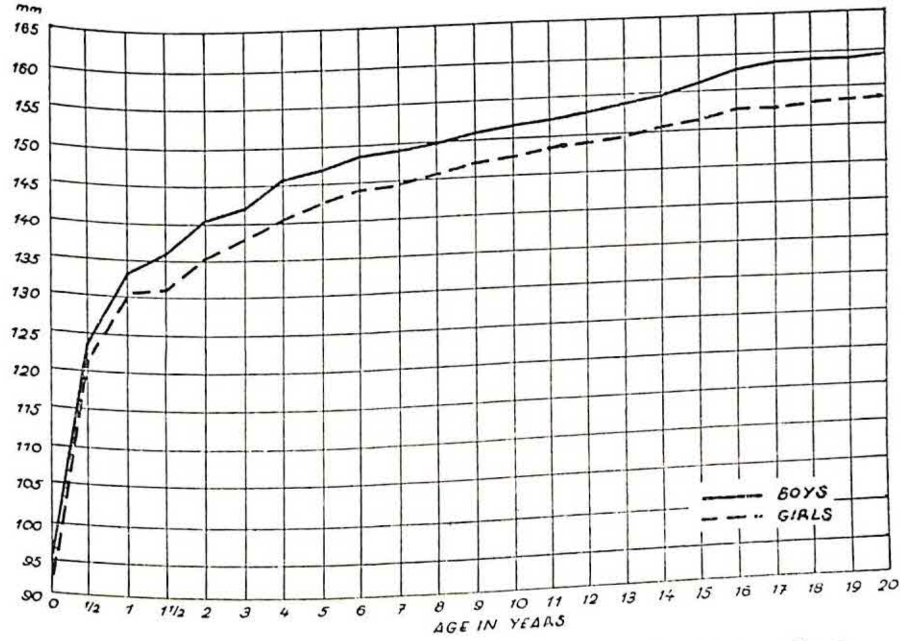

FIG. 3. GROWTH OF HEAD WIDTH FROM BIRTH TO 20 YEARS OF LIFE.

RESULTS AND DISCUSSION OF THE SEPARATE MEASUREMENTS

Head circumference.

The data on head circumference are presented in tables 1 and 2, and

TABLE 1

Head circumference in boys from birth up to 20 years
(in centimeters)

AGE	N	M	S. D.	V	Dif	%
0	215	35.40	1.26	3.58		62.88
½	66	43.48	1.76	4.06	+8.08	77.24
1	70	46.28	1.44	3.13	+2.80	82.21
1½	74	47.42	1.39	2.94	+1.14	84.24
2	71	48.36	1.28	2.65	+0.94	85.91
3	94	49.27	1.37	2.78	+0.91	87.52
4	84	49.95	1.10	2.22	+0.68	88.73
5	130	50.38	1.17	2.32	+0.43	89.50
6	189	51.03	1.47	2.89	+0.65	90.65
7	206	51.58	1.54	2.98	+0.55	91.63
8	202	52.02	1.42	2.40	+0.44	92.41
9	175	52.30	1.41	2.70	+0.28	92.91
10	134	52.87	1.35	2.56	+0.57	93.92
11	110	53.03	1.40	2.65	+0.16	94.20
12	158	53.64	1.54	2.88	+0.61	95.29
13	128	54.02	1.58	2.94	+0.38	95.96
14	126	54.47	1.20	2.21	+0.45	96.76
15	102	54.69	1.36	2.49	+0.22	97.15
16	121	55.08	1.06	1.92	+0.39	97.85
17	104	55.48	1.30	2.36	+0.40	98.56
18	114	55.72	1.27	2.29	+0.24	98.98
19	103	56.08	1.62	2.89	+0.36	99.62
20	101	56.29	1.30	2.31	+0.21	100.00

N—number of subjects
M—mean, cm
S. D.—standard deviations, cm
V—coefficient of variation, %
Dif—yearly increase
%—percentages of the 20-year-old mean

TABLE 2

Head circumference in girls from birth up to 20 years
(in centimeters)

AGE	N	M	S. D.	V	Dif	%
0	209	34.60	1.16	3.36		62.72
½	63	42.24	1.07	2.54	+7.64	76.57
1	82	45.21	1.04	2.30	+2.97	81.96
1½	70	46.32	1.33	2.88	+1.11	83.97
2	63	47.28	1.40	2.97	+0.96	85.71
3	72	48.34	1.21	2.50	+1.06	87.63
4	88	48.96	1.27	2.61	+0.62	88.75
5	147	49.48	1.09	2.21	+0.52	89.70
6	189	50.03	1.24	2.49	+0.55	90.69
7	194	50.91	1.38	2.72	+0.88	92.29
8	179	51.45	1.39	2.70	+0.54	93.27
9	171	51.71	1.37	2.66	+0.26	93.74
10	138	51.93	1.45	2.79	+0.22	94.14
11	108	52.66	1.36	2.59	+0.73	95.46
12	96	52.91	1.35	2.56	+0.25	95.92
13	102	53.64	1.28	2.38	+0.73	97.24
14	130	54.03	1.30	2.42	+0.39	97.95
15	114	54.28	1.21	2.24	+0.25	98.40
16	106	54.39	1.14	2.10	+0.11	98.60
17	121	54.58	1.38	2.54	+0.19	98.94
18	116	54.79	1.42	2.59	+0.21	99.32
19	102	54.94	1.47	2.68	+0.15	99.60
20	103	55.16	1.48	2.69	+0.22	100.00

in graph 1. The difference between the means at birth and 20 years in both sexes show that head circumference increases by about 20 cm during postnatal growth. More than half of this increase occurs during the first year of life, and very little of it after the age of 15 years. Moderate accelerations seem to occur at puberty: that is, at 11 years in girls and 12 in boys.

At birth, the male circumference on the average is 8 mm greater than that of the female. This sex difference persists up to maturity, but at 11 to 14 years, it is temporarily reduced to as little as half of the neonatal value. In this dimension, as in many others, the underlying mechanism is the earlier onset or sexual maturation in girls (Valšik, ’53). Especially at 16 and 17 years, the male increase is markedly greater than that of the female, so that in 20-year-old men this measurement exceeds the female average by 1.13 cm.

When the value at 20 years is expressed as 100%, column 9 in the tables indicates the percentage of the mature percentage attained at each preceding age. At birth, head circumference has already exceeded 62% of the adult value, so that the postnatal increase is less than 38%. In both sexes, 85% of growth is attained by two years of age, 90% by 6, and 95% by 11 or 12 years.

In 1891, Matiegka found that the mean head circumferences of neonates of both sexes in Prague was 34.5 cm. In the present data, the male neonatal mean was 35.4, and the female 34.6. Thus, present infants apparently have slightly larger heads than neonates measured several decades ago.

Our data can usefully be compared with means for 6 and 12 months of age from Hradec Králové (Blecha *et al.*, '53, '54), and from Prague and the district of Pardubice (Šobová, '57), and for two and three years of age in the two later series.

	BOYS				GIRLS			
	6 mo	12 mo	2 yr	3 yr	6 mo	12 mo	2 yr	3 yr
Hradec Králové	43.58	46.88	—	—	42.47	45.44	—	—
Prague	43.70	46.70	48.8	49.8	42.40	45.40	47.6	48.7
Pardubice	43.50	46.90	48.7	50.1	42.10	45.80	47.8	48.8
Brno	43.48	46.28	48.3	49.2	42.24	45.21	47.2	48.3

These comparisons show that the children of Brno have slightly smaller heads than those of the other samples from Czechoslovakia, although all of them are somewhat higher than older data of Czech authors and even of most studies elsewhere (Pfaundler, '16; Quetelet, 1871; Bondyrev, '00 [*cf.* Martin, '28], Orlov, '51). Bayley and Davis ('35), however, reported slightly higher values on American children from California.

Although the national authropometric survey of Czech and Slovak children in 1951 indicated the existence of numerous regional differences in physical development, the present differences may well be pecularities of sampling. While the Brno children from birth to three years of age are mainly from working-day infant homes and from permanent homes for children, those from Hradec Králové, Pardubice, and most of the Prague sample live in their parental homes and were investigated at a consultation institute for children. A more extensive investigation of health and anthropometry would be necessary to find a positive answer to the question whether any marked metrical differences exist between the two categories of children.

At 4 and 5 years of age, comparative data on children from Czechoslovakia are few. At 6–18 years, however, the data of Řehák ('23), on series of boys measured from 1919 to 1922, are closely similar to the present findings. The Slovak children from Trenčin, however, show an average circumference from 0.5 to 1 cm smaller than ours for both sexes and in all age groups (Štampach, '30).

In the present data, the means at all ages for both sexes are a few millimeters higher than those from abroad. These data confirm the statement of Matiegka ('34) that Czechs tend to have comparatively large heads. Indeed, this information is even an item of the national folklore.

In Czechoslovakia, the average head circumference in adult men ranges from 55.5 to 56.5 cm, and in women 54.0 to 55.5 (Valšík, '40; Suchý, '56; Prokopec, '57). Our averages at 20 years in Brno fit in this frame quite well.

Head length and head breadth.

a. *Results.* In considering the average length of the head, tables 3 and 4 indicate that the postnatal increment is 68.6 mm in the males and 64 mm in the females. In both sexes, an increase of about 2 cm occurs in the first 6 months of life, and an additional centimeter is gained in the next 6 months. In the succeeding years, the annual increments are much smaller and sometimes practically negligible. The curve of growth is steepest in the first three years, and is considerably flattened thereafter.

Newborn boys show a head length 3 mm longer than that of girls, and this small difference is statistically significant. It increases to 5 mm in the 10th year, decreases during puberty, and rises substantially after the 14th year. By 20 years, it is more than 20 mm.

At birth, the head length of boys is 63% of the adult length, and in girls it is 64%. It attains 80% of the final value toward the end of the 2nd year in boys, and near the close of the first year in girls. At birth, the head of the female is relatively closer to the adult value, and its postnatal growth is more precocious. Her head reaches 90% of the adult value within 4 years: while with boys it is a year later. Girls reach the 95% level at 10 years, and boys at 13.

Tables 5 and 6 show that the postnatal increase in head breadth in boys is 63.8 mm, and in girls is 60.8. These increments are slightly smaller than those of head length during this interval. About half of these increases occur in the first 6 months of life.

Expressed as percentages of the mature width, the original 60% at birth increases to 80% in the first 6 months, and in boys the increase from 6 to 12 months is 4.5 mm. In girls it is 7.7 mm. In the second year, these increases are also large: 7.6 mm in boys and 6.4 mm in girls. Later the increments become smaller. With boys between 14 and 16, and with girls between 15 and 16 the values again somewhat increase.

TABLE 3

Head length in boys from birth up to 20 years
(in millimeters)

AGE	N	M	S. D.	V	Dif	%
0	215	116.93	4.89	4.18		63.03
½	66	137.75	8.82	3.40	+20.82	74.25
1	70	148.37	6.98	4.70	+10.62	79.98
1½	74	154.18	6.85	4.44	+5.81	83.11
2	71	158.53	6.38	4.02	+4.35	85.46
3	94	162.98	7.08	4.34	+4.45	87.85
4	84	166.52	5.82	3.49	+3.54	89.76
5	130	167.40	5.25	3.13	+0.88	90.24
6	189	168.65	4.64	2.75	+1.25	90.91
7	206	170.73	6.33	3.71	+2.08	92.03
8	202	171.44	5.87	3.42	+0.71	92.42
9	175	171.87	6.16	3.58	+0.43	92.65
10	134	173.71	5.84	3.36	+1.84	93.64
11	110	173.70	5.83	3.36	—0.01	93.63
12	158	174.39	5.47	3.13	+0.69	94.01
13	128	177.40	5.78	3.28	+3.01	95.63
14	126	177.85	5.33	3.00	+0.45	95.87
15	102	180.40	6.10	3.31	+2.55	97.25
16	121	182.16	6.12	3.35	+1.76	98.19
17	104	183.26	5.25	2.86	+1.10	98.79
18	114	184.17	6.62	3.59	+0.91	99.28
19	103	185.27	5.57	3.00	+1.10	99.87
20	101	185.50	6.00	3.23	+0.23	100.00

Head breadth attains 90% of its adult value between 3 and 4 years of age, and 95% after the ninth year of life. These limits are reached somewhat earlier than the corresponding limits of head circumference and head length.

In all age categories, boys have absolutely broader heads than girls, but their heads are not broader relative to head length. The absolute sex difference in breadth varies from 3 to 6 mm. With increasing age, it also increases slightly, so that at birth it is 3 mm, and at 20 years it is 5.5 mm.

TABLE 4

Head length in girls from birth up to 20 years
(in millimeters)

AGE	N	M	S. D.	V	Dif	%
0	209	113.88	4.80	4.21		64.04
½	63	133.42	8.12	6.08	+19.54	75.03
1	82	146.47	6.68	4.56	+13.05	82.37
1½	70	150.05	6.05	4.03	+3.58	84.39
2	63	154.68	6.66	4.30	+4.63	86.99
3	72	159.23	5.47	3.43	+4.55	89.55
4	88	162.22	5.34	3.29	+2.99	91.23
5	147	163.87	5.54	3.38	+1.65	92.16
6	189	165.34	5.52	3.34	+1.47	92.99
7	194	166.04	5.82	3.51	+0.70	93.38
8	179	167.40	5.41	3.23	+1.36	94.15
9	171	168.45	5.03	2.98	+1.05	94.74
10	138	169.09	5.40	3.19	+0.64	95.10
11	108	171.76	4.63	2.69	+2.67	96.60
12	96	172.18	5.98	3.47	+0.42	96.83
13	102	174.35	5.38	3.08	+2.17	98.05
14	130	175.32	5.52	3.15	+0.97	98.60
15	114	175.90	5.63	3.20	+0.58	98.93
16	106	177.09	5.84	3.29	+1.19	99.56
17	121	176.60	5.76	3.26	—0.49	99.32
18	116	177.12	6.00	3.38	+0.52	99.61
19	102	177.58	5.91	3.32	+0.46	99.87
20	103	177.80	5.70	3.20	+0.22	100.00

b. *Discussion.* The length and width of the head determine the values of the cephalic index. They are therefore among the features whose averages largely depend on the frequency of "subracial" types within any populations which we may wish to compare. We must always keep this circumstance in mind in comparing our values of head length and breadth with those from other parts of Europe and elsewhere, especially if they are geographically far distant.

Pachner studied the length and width of the neonatal head in

Czechs from Ostrava. His series comprised 88 boys and 62 girls. His findings were published by Borovansky ('37):

	Boys	Girls
Head length	114.4 mm	111.0 mm
Head breadth	93.1 mm	90.8 mm.

TABLE 5

Head width in boys from birth up to 20 years
(in millimeters)

AGE	N	M	S. D.	V	Dif	%
0	215	95.80	4.88	5.10		60.21
½	66	128.63	7.86	6.11	+32.83	80.84
1	70	133.14	5.33	4.01	+4.51	83.68
1½	74	136.13	4.87	3.57	+2.99	85.56
2	71	140.77	5.83	4.14	+4.64	88.47
3	94	142.39	5.75	4.04	+1.62	89.49
4	84	145.35	4.41	3.04	+2.96	91.35
5	130	146.37	5.34	3.65	+1.02	91.99
6	189	148.36	5.67	3.82	+1.99	93.24
7	206	148.93	5.10	3.43	+0.57	93.60
8	202	149.83	4.72	3.15	+0.90	94.17
9	175	150.61	5.03	3.34	+0.78	94.66
10	134	151.48	4.90	3.23	+0.87	95.21
11	110	151.92	5.24	3.45	+0.44	95.48
12	158	152.43	5.37	3.52	+0.51	95.80
13	128	153.00	5.49	3.59	+0.57	96.16
14	126	154.52	4.66	3.01	+1.52	97.12
15	102	155.90	6.20	3.97	+1.38	97.98
16	121	157.64	5.47	2.83	+1.74	99.08
17	104	158.40	5.44	3.43	+0.76	99.56
18	114	158.80	5.44	3.42	+0.40	99.81
19	103	158.23	5.43	3.43	—0.57	99.45
20	101	159.10	6.10	3.83	+0.87	100.00

These values on the whole are 2 to 3 mm (2–2½%) lower than our means. His sample came from the Maternity Hospital at Ostrava, where the patients were mainly derived from the less prosperous social classes. The health of this group of infants was not especially favorable (Pachner, personal communication, 1954). Another more plausible factor may be the secular increase of size in the newborn children of today. On the average, they are becoming heavier, longer, and with an increased size of the thorax and head. Our own observations corroborate this trend in the head.

From birth to 6 years, head measurements are too scarce in Czech and Slovak children to permit us to compare our results with data of other authors.

TABLE 6

Head width in girls from birth up to 20 years
(in millimeters)

AGE	N	M	S. D.	V	Dif	%
0	209	92.80	4.19	4.51		60.41
½	63	122.47	6.38	5.21	+29.67	79.73
1	82	130.19	6.08	4.67	+7.72	84.75
1½	70	131.37	5.93	4.51	+1.18	85.52
2	63	135.65	5.77	4.25	+4.28	88.31
3	72	138.20	4.26	3.08	+2.55	89.97
4	88	140.31	4.58	3.27	+2.11	91.34
5	147	142.90	5.07	3.54	+2.59	93.03
6	189	144.34	5.40	3.74	+1.44	93.97
7	194	144.83	5.00	3.45	+0.49	94.29
8	179	145.55	4.57	3.14	+0.72	94.75
9	171	147.32	5.14	3.49	+1.72	95.91
10	138	147.54	5.99	4.06	+0.22	96.05
11	108	148.62	4.88	3.28	+1.08	96.75
12	96	148.97	4.62	3.10	+0.35	96.98
13	102	149.51	4.23	2.83	+0.54	97.33
14	130	150.23	5.47	3.64	+0.72	97.80
15	114	151.50	5.40	3.56	+1.27	98.63
16	106	153.04	6.09	3.97	+1.54	99.63
17	121	152.79	5.59	3.65	—0.25	99.47
18	116	153.06	5.59	3.65	+0.27	99.64
19	102	153.07	6.64	4.22	+0.01	99.65
20	103	153.60	5.83	3.79	+0.53	100.00

As mentioned earlier, our data from 6–18 years compare closely to those of Řehák ('23), on Czechs, and of Štampach ('30) for Slovaks. Hence, we may conclude that no secular increase in the size or shape of the head has occurred in Czechoslovakians of school age in the past three or 4 decades.

It is not easy to compare our findings on adolescent boys with the data of Doškova ('23), for her statistics describe a comparatively small number of individuals. Her sample is divided into two quite distinct groups, both socially and metrically. One of her groups consists of bakers' apprentices, and the other is a series of grammar-school boys. In the 14th and 15th year, the grammar-school group display higher values of these measurements than do our sample from Brno. These differences tend to disappear at 16 and 17. The heads of the apprentices are substantially smaller, but shorter and broader at the same time—a finding of no small interest.

Matiegka ('27) mentions that the length of the head in Czech boys increases 12 mm from 6 to 14 years of age: while the breadth increases 6 mm. The boys in Brno show practically this same increase in breadth (6.1 mm), but their increase in head length is only 7.2 mm. In the girls of Brno, we found that length increased 9.9 mm, and breadth 5.8 mm. Even if the values of different series vary, we can state that in all cases the length of the head increases more rapidly than the width. This difference results in a relative elongation of the head in general, so that the cephalic index falls by 1–2 units.

When we compare the values of head length and breadth for the immature population of Brno with corresponding data published abroad, we confirm the previous conclusion of Řehák ('23). He observed that in head length, Czech children especially resemble German children from Dresden, as measured by Röse (Martin, '28). The Polish boys from Cracow (Jasicki, '38) and from Poznan (Czaplewski *et al.*, '33-34) have averages which are also fairly close to ours. The head breadth of the children of Brno at all ages are among the highest in Europe, which also confirms the previous observations of Řehák ('23) and of Matiegka ('27). These authors assert that at all ages, the Czechs tend to have shorter, broader heads than other nationalities.

Measurements of the head in American children differ considerably from those of Czechs. The average length at all ages in American Whites is some 10 mm greater, and the breadth 5–7 mm narrower (West, 1893; Meredith, '53).

The average values of head length and breadth in various samples of adult Czechs lie within the following ranges:

	Men	Women
Head length (mm)	184–186	176–178
Head breadth (mm)	156–159	149–153.

The values at 20 years of age for our sample from Brno lie at the upper limits of these ranges. By comparison with world distributions of means of these measurements (Martin, '28; Lebzelter and Saller, '23 [*cf.* Martin, '28]), the inhabitants of Brno have head lengths in the midrange, and the breadth are relatively large.

The range and standard deviations of our three measurements at all ages indicate that variability is greater in boys than in girls. The coefficients of variation are highest at birth and in the first year of life, when they range from 4–6%. In the next two years, these coefficients fall moderately, and are stabilized during the remaining years at 2–4% for circumference, and 3–4% for length and width of the head.

The standard deviations and coefficients of variation from our study are rather low and do not differ greatly from the values of other authors. These findings indicate that the series was sufficiently homogeneous and the data reasonably free from errors of observation.

CONCLUSION

According to Merkel (1882), the postnatal growth of the human head progresses most intensely in two periods: birth to 7 years, and from pubescence to the middle teens. This first phase is mainly a reflection of the rapid growth of the brain at this time. The period from 7 years to pubescence is one of relative stagnation. The subsequent acceleration during adolescence is most conspicuous in the circumference and length of the head, and lasts until the 15th or 16th year. Subsequent growth in all dimensions is minimal, and does not exceed 1–2% of the total.

Even at birth, all the dimensions of boys are greater than those of girls. This difference is evident throughout growth, except for its diminution during the earlier pubertal acceleration of growth in girls. The head and other parts of the body stop growing earlier in girls. For example, all three head measurements attain about 99% of their mature values at the age of 15 years: while growth is more or less uninterrupted in boys from 16 to 20 years.

Our values for 20-year-old males and females in Brno may be taken as near the final average for adults in Brno. According to Pfuhl ('28 [in: Peter *et al.*, '28]), the continuing growth of the head past this age does not exceed 2–4 mm. It is chiefly explained by the accumulation of subcutaneous adipose tissue, and perhaps by an increasing bulk of the muscles of mastication—especially the temporals. Very little of it is produced by growth of the skull. These precesses are noticeable during the age range of 20–30 years, but after 35 years some populations exhibit a slight reduction, so that the average figures at 40–50 years are almost identical with those at 20 years.

SUMMARY

The purpose of this paper is to describe the growth of three dimensions of the head from birth to maturity. These measurements included circumference, length and breadth. The sample measured consisted of 2,879 boys and 2,763 girls of Czech nationality from Brno, Czechoslovakia. The data indicated that from birth through the pre-school years, the present sample had somewhat larger heads than their predecessors 30–60 years ago. These differences disappear by 6 years of age. We may therefore conclude that above this age, the dimensions of the head in Czechs are comparatively stable and do not as yet exhibit any evolutionary changes of the sort which have occurred in some populations elsewhere in the body.

Within Czechoslovakia at least, these measurements may serve as useful norms for the estimation of physical development of children—especially in conjunction with recently published tables of stature and weight.

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BOOK REVIEWS

Interdisciplinary Team Research: Methods and Problems. By MARGARET BARRON LUSZKI. xxvii + 355 pp. New York University Press on behalf of National Training Laboratories, New York, 1958. $6.00.

Why has interdisciplinary research become so fashionable in recent times? Are there particular kinds of problems that require interdisciplinary, as distinguished from unidisciplinary, effort? If so, what are the criteria whereby we may judge the probable success or failure of one approach in preference to another? What factors bring about frictions between members of different disciplines when they work together in research? Are these frictions due to the nature of training, varying research strategies, different philosophical orientations, the "group personality" of individual members, or what? May we distinguish various patterns of collaboration which interdisciplinary efforts can take? What are the crucial elements which influence organization and leadership of research teams? What sorts of situations bring about effective intra- and extra-team relations?

These and a host of other questions are intensively explored in this volume. It is a summary of ideas and observations presented in five interdisciplinary work conferences exploring research problems in the area of mental health. Over 100 invited individuals from some 60 different universities and public and private agencies, and from the fields of anthropology, medicine, psychiatry, physiology, social work, psychology and sociology participated in these conferences sponsored by the National Training Laboratories and supported by the National Institute of Mental Health. The conferences, held in conjunction with national meetings of the major mental health disciplines, were tape-recorded. Dr. Luszki has done a superb job of organizing and editing a formidable amount of verbatim material, and has produced a highly readable volume on an important aspect of the present day American research scene, to be read with profit by individuals in the behavioral as well as other sciences.

The book presents some close and interesting parallels with the experience of the reviewer in administering the Interdisciplinary Program in the Behavioral Sciences sponsored by the Air Force Office of Scientific Research at the University of New Mexico. Problems such as the status hierarchy of various disciplines, difficulty in communication across displinary boundaries, what sort of research (basic or applied) is more successfully attacked via a multidisciplinary approach etc., were common in both the New Mexico and NTL Conferences.

An aspect of the book which on the one hand is both refreshing and illuminating, and which at the same time may be quite misleading is the extensive portrayals by the conferees of themselves and of members of other disciplines with which they work. The uncritical reader may be misled by some highly personalized, stereotyped, or oversimplified pictures. For example, an anthropologist, discussing his perceptions of his own discipline, say, on p. 55, "Most of us are children, grandchildren, or great-grandchildren of Boas," and further, on p. 57, the same individual says, "Very little of anthropology is written. About ninety per cent of it exists in the minds of anthropologists. . . ." Later, on p. 63, we have a psychiatrist exclaiming, "My profession, for better or worse, is antitheoretical," a statement which would hardly be concurred with by many other psychiatrists who, for better or worse, adhere slavishly to fixed theoretical dogmas. And again, although the reviewer, as a psychologist, agrees with the general portrait of the psychologist expressed by other disciplines, he would hardly assent with the feeling one obtains from the book that psychologists are concerned almost exclusively with "right little, tight little" problems.

Despite occasional digressions into what the reviewer believes are irrelevancies (mostly in an attempt, I think, to be faithful to the transcribed records) Dr. Luszki has presented materials of fundamental interest to persons engaged in or administering interdisciplinary research. A feature of great practical value is a presentation of "guide lines" for such individuals, only two examples of which will be cited here. One is a list of "Characteristics of Close Collaboration," constituting some two dozen factors delineated from the standpoints of the problem, theory, methodology, and group functioning, by which one may judge how intimate any interdisciplinary effort really is. Another is a set of principles for recruiting and keeping effective personnel, involving considerations of timing, choice of disciplines, physical facilities, etc.

An important addition to the book is a 348-item bibliography, selected from a wide variety of sources and slanted toward a coordination with various chapters. Although much of this bibliography bears upon interdisciplinary problems in the field of mental health, there are enough general references to interest the non-specialist in this area.

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Hemophilia and Hemophilioid Diseases. Edited by KENNETH M. BRINKHOUS. xxii + 265 pp. The University of North Carolina Press, Chapel Hill, 1958. $7.50.

The editor of these proceedings of an international symposium held in New York on the 24th and 25th of April, 1956 tells us that "This volume was prepared for publication with the idea that it would provide a kaleidoscopic view of current ideas of the broad field of hemophilia, with the hope that this would be valuable both to the investigator in his further explorations in this complex field, and to the practitioner in his contact with the multiple medical, psychological and socio-economic problems facing patients and their families." This important volume does all that the editor hoped for it. It does more. No one can read the illuminating papers and the discussion which follows each section without coming away with the strong feeling that it is but a matter of a relatively short time before there will be a major breakthrough in the successful treatment of this unhappy disorder. Meanwhile much intensive research requires to be done. The research is being done, and the present volume is the best available account of the findings that have been made to date.

The book is divided into 7 parts, with a total of 29 papers. In the first part the clotting defect in hemophilia, the antihemophilic factor, its purification, assay and standardization, are first discussed. It is 47 years since Addis, in 1911, reported his experiments on the attempt to purify AHF. We are clearly nearer success in achieving its isolation. The evidence strongly suggests a protein of a high molecular weight.

Inhibitors, in the form of platelet cofactors, anticoagulants, plasma refractoriness, and antibodies, are examined as possible factors in different groups of individuals suffering from hemophilia. In the first part, insufficiency of hemostasis, and also defective platelets and consequent deficient development of thromboplastin are examined as causes of hemophilia. From the cases offered as examples it appears that there exist hemophilias that behave much like the classical hemophilias, and are inherited in the same way, but restricted to different elements of the blood. Defects in platelet formation, in thromboplastin, prothrombin or in factors operative upon these, or AHF, and more, are all under suspicion. These matters are examined in the second section on the hemophilioid states, in which parahemophilia (proaccelerin deficiency) is stimulatingly discussed by Fantl, and some attempt at order is introduced into a terminology which is almost as bad as that which exists in the field of palaeoanthropology. Proconvertin, and its absence in a patient, is beautifully analysed by Goldstein and Benjamin, as is the plasma thromboplastin component deficiency by Aggeler. Geneticists and anthropologists will regret that the letters PTC among hematologists stand for plasma thromboplastin component since in human biology they have for many years represented phenyl-thio-carbamide in relation to its specific tasting genes. Rosenthal continues his important reports on plasma thromboplastin antecedent deficiency, a disorder which occurs in both males and females, in whom it is transmitted as a simple autosomal dominant with variable degree of expression and penetrance. Ratnoff and Margolius report the Hegeman trait, which is a familial disorder of blood coagulation characterized by great prolongation of clotting time of venous blood, but without a tendency to bleeding. The disorder is due to an autosomal recessive gene.

Part three contains a great deal more than a presentation of the genetic facts. No one who writes about hemophilia can neglect to make himself acquainted with Graham's paper on genetic problems. The next paper by Pavlovsky, "The marriage problem and descendants of hemophiliacs" is among the most significant ever written on the matter of sterilization of "the unfit." It adds greatly to the value of this volume.

Part four deals with the diagnosis of hemophilia, in an excellent presentation by Quick, followed by a valuable contribution to our understanding of the defect in the clotting mechanism by de Nicola and treatment with lysozyme, and an account of electrophoretic studies of hemophilic plasma and serum by Stefanini and Moschides, in which a significant electrophoretic anomaly is demonstrated in a number of hemophiliacs as well as in hemophilia carriers.

In Part five the management of hemorrhage in hemophilia and related diseases is dealt with in four separate papers, not the least least valuable of the contributions appearing in the discussion, during which one of the scientists attending the symposium, himself a hemophiliac, describes his own successful self-treatment.

In Part six hemophilic arthropathy is considered in all its practical phases, and in Part seven, the psychiatric and socioeconomic aspects of hemophilia are considered. Each of these contributions is excellent.

It remains but to be added that Dr. Brinkhous has done a magnificent editorial job, all the diagrams are beautifully clearly and uniformly produced, and the University of North Carolina Press has maintained its usual high standard of book production. Altogether this is a memorable volume.

ASHLEY MONTAGU

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The Population Ahead. Ed. by ROY G. FRANCIS. 160 pp. University of Minnesota Press, Minneapolis, 1958. $3.75.

Interdisciplinary symposia tend to have a high density of stimulating ideas at the expense of intensive and comprehensive treatment of any one line of analysis. This small volume, based upon papers presented at a conference on population problems held at the University of Minnesota in 1957, is no exception.

Nine authors from fields as diverse as economics, geography, and zoology make contributions bearing the distinctive mark of their specialization. Such topics as minimum subsistence, population equilibrium, optimum rates of growth, and the genetic future of man are but a sample of the many facets of population discussed.

While concern with population growth as a social problem is identified as peculiar to Western thought and culture, no contributor denies the perils of unchecked population growth to men and societies. In order to support a growing populace at current standards, large portions of national incomes must be reinvested to maintain and increase productivity. Substitutes will have to be found for depleted energy and mineral recources. The man of the future may have to be a herbivorous animal with a taste for Chlorella or water hyacinth.

Some contributors see these proposals as only temporary expedients, with artificial control of fertility as an "urgent need." The arithmetic of population growth raises the hypothetical possibility that before many generations even standing room may become a scarce commodity. Planning for future population demands, and controlling population in both numbers and genetic quality are seen as necessities if the material and intellectual standards are to be maintained or improved.

Readers, stimulated by the papers and commentaries of 6 additional participants in the conference, are likely to find the volume of little help in directing them to additional materials. Only a few references are made to published sources and in some instances these are not fully identified. No bibliography is presented.

PHILIP C. SAGI

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The Reproductive Development of the Female with especial Reference to the Period of Adolescent Sterility. By M. F. Ashley-Montagu. xviii + 234 pp. Julian Press, New York, 2nd ed., 1957. $5.00.

The first edition of this work appeared in 1946 under the title, *Adolescent Sterility.* This, the second edition, has been rewritten and expanded into a most useful summary of the anthropological, biological, and other literature bearing on the question of adolescent sterility in the human female and of a comparable period of infertility in a variety of animals.

The author reviews the published reports of early ethnologists who had observed that, in societies in which adolescent girls were permitted considerable sexual freedom, they seldom bore children until several years after their first menstruation, despite frequent sexual intercourse with mature males in the interim. C. G. Seligman had noted this in his study of the Southern Massim of New Guinea, the results of which were published in 1910, in his book, *The Melanesians of British New Guinea.* The same observation was made by J. H. Hutton, about ten years later, among the Angami Nagas of Assam; in 1926, by W. H. R. Rivers, among the Eddystone Islanders of Melanesia; by Malinowski, among the Trobriand Islanders of Northwestern Melanesia; and by a number of more recent investigators. Malinowski's speculation about the existence of "some physiological law which makes conception less likely when women begin their sexual life young" has been amply supported by the definitive studies of the reproductive processes in the Rhesus monkey and in the human female by Carl Hartman, whose paper "On the relative sterility of the adolescent organism" was published in 1931.

Though this book consists largely of a series of summaries and critical evaluations of pertinent publications, the author's facile style successfully avoids the monotony which is often associated with the presentation of such material. The text is enlivened, too, by occasional provocative comments, such as the gem on p. 72. In discussing Raven's report that he had noted swelling of the sexual skin of an adult female gorilla which he encountered in the Lake Kivu area of the Belgian Congo, the author raises the intriguing question as to whether or not 40 yards (the distance at which Raven saw this animal) is a satisfactory distance at which to judge the state of the genital region of a departing female gorilla!

In the opinion of the reviewer, Dr. Ashley Montagu's book is an important contribution to the anthropological and related biological literature and fully deserves the wide circulation which it is certain to receive. It should remain for many years the definitive work in its field.

WILLIAM WALTER GREULICH

Stanford University

BOOK NOTICES

DIFFERENTIAL PSYCHOLOGY: INDIVIDUAL AND GROUP DIFFERENCES IN BEHAVIOR. By Anne Anastasi. xii + 664 pp. Macmillan, New York, 1958. $7.50. Third edition, streamlined, of a widely used textbook presenting the history of the field, the nature and extent of individual differences, and the contribution of heredity, training, age, sex, social class, race and culture. Separate chapters deal with Physique and Behavior, and with Constitutional Types.

LOYALTY AND SECURITY. By Ralph S. Brown, Jr. xvii + 524 pp. Yale University Press, New Haven, 1958. $6.00. Comprehensive treatment of "employment tests in the U. S.," seen as a part of the larger and persisting problem of reconciling the needs of national security with the claims of individual freedom.

MANUAL DE ANTROPOLOGÍA FÍSICA. By Juan Comas. 689 pp. Fondo de Cultura Económica, Mexico, D. F., 1957. No price indicated. The reviewer regrets that it was not feasible to provide a full-length book review of this comprehensive textbook which grew out of the author's course on general physical anthropology in Mexico's Escuela Nacional de Antropología.

STATISTICAL ANALYSIS. By Allen L. Edwards. xii + 234 pp. Rinehart & Co., New York, 1958. $4.00. Accompanied by a Workbook. 76 pp. $1.25. A well written textbook for introductory course in statistics, emphasizing that the student should gain an understanding of statistical theory and learn how statistical methods can be used to answer questions within his subject-matter specialty (psychology and education, in the present case).

A COMPREHENSIVE DICTIONARY OF PSYCHOLOGICAL AND PSYCHOANALYTICAL TERMS. By Horace B. English and Ava Champney English. xiv + 594 pp. Longmans, Green and Co., New York, 1958. $8.00. "This is a defining dictionary, a collection of meanings, not an encyclopedic compilation of facts." A result of one man's labor extending over 30-odd years, benefiting from the counsel and other assistance of many.

STRESS AND STRAIN IN BONES. By F. Gaynor Evans. x + 245 pp. C. C. Thomas, Springfield, Ill., 1957. $6.50. The author integrates the results of his extensive experimental work on biochemical and physical properties of bones, much of it made in cooperation with the Department of Engineering Mechanics, Wayne State University, with information reported in the literature on the stress-strain phenomena in bones and their relation to fractures and osteogenesis.

MILLIONS STILL GO HUNGRY. FOOD AND AGRICULTURE ORGANIZATION OF THE UNITED NATIONS. x + 102 pp. Columbia University Press, New York, 1957. $1.00. It is estimated that 120,000 new mouths to feed are added daily to the world's population. This makes the present report on the FAO activities of more than passing interest.

IMPACT OF THE ANTIBIOTICS ON MEDICINE AND SOCIETY. Edited by Iago Galdston.

x + 222 pp. International Universities Press, New York, 1958. $5.00. Second in the series issued by the Institute of Social and Historical Medicine (The New York Academy of Medicine), the monograph deals with one of the most spectacular advances in medical science. The volume, a handiwork of 14 authors, is a unique compendium, focussed on the advent of antibiotics but not neglecting their present-day significance (*cf.* a demographic analysis of recent changes in mortality and morbidity).

MIRROR TO PHYSIOLOGY. By R. W. Gerard. xi + 372 pp. American Physiological Society, Washington, D. C., 1958. No price indicated. A detailed survey of physiological science, regarded broadly as "dynamic (functional) biology," was undertaken by the American Physiological Society. The report covers activities and income of physiologists, how they like what they do, where they get their training, the geographical distribution of employed physiologists, characteristics of physiological research and its financial support.

INTRACLASS CORRELATION AND THE ANALYSIS OF VARIANCE. By Ernest A. Haggard. xvii + 171 pp. Henry Holt & Co. (Dryden Press), New York, 1958. $2.90. The concept of intraclass correlation, going back to the work of J. A. Harris (1913), is integrated with the modern theory of the analysis of variance. Applications to some problems encountered in the study of behavior (reliability stuides, pattern analysis, treatment of data in the form of ranks) are described.

THEORIES OF PERSONALITY. By Calvin S. Hall and Gardner Lindzey. xi + 572 pp. John Wiley & Sons, New York, 1957. $6.50. A survey of the major contemporary theories of personality, presented essentially in expository rather than critical terms.

MEDICAL SOCIOLOGY. By Norman G. Hawkins. xx + 290 pp. C. C. Thomas, Springfield, Ill., 1958. No price indicated. Christened by Charles McIntire (1894), a physician, the field began to develop intensively in the last decade. This is an original attempt to give it a theoretical basis, identify its content, and present the methods of analysis.

DETERMINISM AND FREEDOM IN THE AGE OF MODERN SCIENCE. Edited by Sidney Hook. xv + 237 pp. New York University Press, New York, 1958. $5.00. "A philosophical symposium," concerned with a perennial issue. "Science" was taken, expressly by Harvard's Percy W. Bridgman and, tacitly, by the symposium, as being "pretty nearly equivalent to physics."

ULTRASOUND IN BIOLOGY AND MEDICINE. Edited by Elizabeth Kelly. vii+ 243 pp. American Institute of Biological Sciences, Washington, D. C., 1957. No price indicated. These proceedings of a symposium sponsored by the Bioacoustic Laboratory of the University of Illinois and the Physiology Branch of the Office of Naval Research bring out the varied uses of ultrasound, from the development of flowmeters for the measurement of blood velocity (and visualization of soft tissues, paralleling to some extent roentgenography) to production of lesions by ultrasonic irradiation of tissues and neurosonic surgery.

HUMAN DISSECTION: ITS DRAMA AND STRUGGLE. By A. M. Lassek. x + 310 pp. C. C. Thomas, Springfield, Ill., 1958. $6.50. History of human "anatomizing," from the pre-Alexandrian phase to its present status in the U. S. Much of the saga revolves around the procurement of cadavers. The scientific study of the gross structure of man—the author believes—"more than other factors, has gone a long way toward neutralizing superstition, uncertainty and mysticism in medicine."

UNIQUENESS OF THE INDIVIDUAL. By P. B. Medawar. 191 pp. Basic Books, New York, 1957. $4.00. A collection of articles and lectures, centered around the problems of evolution. Of special interest is the chapter that gave the title to the volume, as well as the chapters on the Imperfections of Man, and on the role of Tradition ("exosomatic evolution").

BIOTIC WORLD AND MAN. By Lorus J. Milne and Margery Milne. xiii + 530 pp. Prentice-Hall, Inc., Englewood Cliffs, N. J., 1958. $7.95. Well written, richly illustrated introductory textbook of general biology, not neglecting Man.

PSYCHOLOGIC STUDY OF MAN. By John Money. xi + 216 pp. C. C. Thomas, Springfield, Ill., 1957. $4.75. An attempt to have a fresh look at human personality. An unorthodox, all right, contribution to psychodynamic theory but hardly the right menu for beginning students, and not lacking some exasperating features (*cf.* the repeated statement that human species is not asexual, pp. 11, 199) stemming, in part, from the author's neologistic fertility.

SCIENTIFIC BASIS OF ATHLETIC TRAINING. By Laurence E. Morehouse and Philip J. Rasch. ix + 238 pp. W. B. Saunders, Philadelphia, 1958. No price indicated. Considers biological factors (including sex and age) affecting peak human performance. Second part deals with athletic injuries (protection, first aid, rehabilitation after injury).

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INFORMATION FOR CONTRIBUTORS

Human Biology, a record of research, is a quarterly journal devoted to the subject its name implies. It publishes original articles dealing with human genetics, development, growth, reproduction and aging, functional anatomy, bio-anthropology, human ecology, population dynamics, etc.

The original manuscripts, typed double-spaced on one side of 8½ x 11 inch paper in complete and finished form precisely as they are to appear, should be sent—packed flat—to the editor, Dr. Gabriel Lasker, Wayne State University College of Medicine, 1401 Rivard Street, Detroit 7, Michigan. It is desirable that a second copy be included in order to expedite prompt consideration, and that an additional carbon copy and copies of all illustrations should be retained by the author.

Where possible, illustrations should be made for reproduction as line engravings, hence drawn in India ink as boldly as possible. Lettering must remain legible after figures are reduced to fit on 4½ x 6¾ inch pages (letters 1½ to 2 mm high after reduction are adequate). Figures should be numbered consecutively with Arabic numerals. Tables should be numbered independently from 1 up, and both figures and tables should have captions. When the amount of tabular and illustrative material is judged to be excessive or unusually expensive, authors may be requested to pay the excess cost. Galley proofs will be sent to the author and must be returned promptly; when the author makes substantial departures from copy, the cost will be charged to him. Three copies of an abstract (not to exceed 225 words) should be submitted when the proof is returned. One will be sent to the editor of *Biological Abstracts.*

References in the text to the literature should be made by using the author's name followed by the year of publication: "Studies of Garn and Saalberg (1953) . . ." At the end of the article, under the heading "Literature Cited," full references with titles and inclusive page references should appear (double spaced) in alphabetical order of the authors' names: "Garn, S. M. and J. H. Saalberg 1953 Sex and age differences in the composition of the adult leg. Human Biol., *25:* 144-153."

Authors will receive 25 free reprints. Orders for additional reprints should be placed at the time proof is returned. It will not be possible to accept orders for reprints after the proof has been returned to the press.

Human Biology

a record of research

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relations, if any, between fertility and the various physical, psychological, and social attributes of man. The present paper consists of a preliminary analysis of the measurements and information obtained by the study which pertain to the relations between fertility and certain physical characters.

ACKNOWLEDGMENTS

The Assortative Mating Study was supported from 1950 to 1955 by a generous grant to the University of Michigan from an anonymous donor. The field and laboratory work of the Study were conducted by the Institute of Human Biology under the direction of Lee R. Dice. The statistical computations for this paper were completed while the senior author was employed during the summer of 1958 by Cranbrook Institute of Science under a grant (M-1650) from the National Institute of Mental Health.

THE SAMPLE

The individuals whose measurements and fertility are analyzed in this paper were selected at random from the adult population of Ann Arbor, Michigan, in two stages. Males were considered to be adult if they were at least 18 years old; females if they were at least 17 years old. The primary sampling units were defined in terms of number of dwelling units, such that each primary unit contained at least 10 and at most 20 dwelling units. The city of Ann Arbor was divided into 699 such primary sampling units, and of these 108 were selected at random in the first stage of the sampling. A field census was then made of all the individuals resident in each primary unit selected for study. The individuals were classified into married pairs, single females, and single males. A single individual was defined as one not married at the time of the census, regardless of whether or not he or she had previously been married. From each of these three categories selections were made at random, as follows:

1. Married pairs, selection rate $\frac{1}{4}$
2. Single females, selection rate $\frac{1}{4}$
3. Single males, selection rate $\frac{1}{2}$.

The selection rate for single males was made larger than that for single females because there were fewer single males than single females in the population.

The selections were made during the years 1951-1954, inclusive. Every effort was made to make the sample as random as possible. However, inasmuch as about a quarter of the individuals selected for study refused to participate, the sample cannot be regarded as wholly random. Only persons of European descent were used in this aspect of the study. Females less than 20 and males less than 25 years of age, although present in the sample, were not included in this study because they exhibited almost no variation in fertility.

The complete sample used in this study consisted of 97 single females and 227 married females, 43 single males and 187 married males.

FERTILITY

The term fertility, as employed in this paper, pertains to the total number of children which an individual acknowledges to have produced. It has no reference to innate reproductive capacity or to the multitudinous factors underlying variation in reproductive performance. Our concern here is entirely with actual number of children produced, as it is this factor which is most relevant to the future genetic composition of the population. Consequently, in investigating the relationship between fertility and body measurements, single and married individuals are combined in the same sample.

Another reason for ignoring marital status in the present study is the considerable difficulty in accounting for it. It is obviously not sufficient merely to classify people as married or not married, for individuals differ in the length of time they have been married. Moreover, the significance, so far as fertility is concerned, of a year of married life depends on the age of the individuals at the time. A year of marriage commencing at age 20, for example, is more likely to result in the birth of a child than is a year of marriage commencing at age 50. Furthermore, the importance of a year of marriage even for individuals of the same age varies with their economic condition at the time. These considerations, combined with the fact that many individuals have been married several times, make it exceedingly difficult to take this factor into consideration in studying fertility. And besides, as pointed out in the previous paragraph, it is not necessary to consider it so far as the genetic and evolutionary implications of the data are concerned.

The 324 females were classified into 11 age groups and the 230 males into 9 age groups. The fertility of each individual was then classified as either above average or below average, depending upon whether the number of children acknowledged by the individual was greater than or

TABLE 1

Age and fertility categories for males, showing the number of single and married individuals in each of the 18 categories, the mean fertility per individual and the range of fertility per individual for each category

	BELOW AVERAGE FERTILITY				ABOVE AVERAGE FERTILITY			
	NUMBER OF INDIVIDUALS		MEAN FER-TILITY	RANGE OF FER-TILITY	NUMBER OF INDIVIDUALS		MEAN FER-TILITY	RANGE OF FER-TILITY
AGE	SINGLE	MARRIED			SINGLE	MARRIED		
25–29	8	20	0.0		0	25	1.4	1–2
30–34	3	17	0.4	0–1	0	17	2.5	2–4
35–39	2	7	0.3	0–1	0	14	2.6	2–4
40–44	1	6	0.4	0–1	0	9	2.4	2–4
45–49	0	13	1.1	0–2	0	8	3.2	3–4
50–54	6	4	0.1	0–1	1	12	2.9	2–4
55–59	3	4	0.6	0–1	1	12	2.6	2–4
60–69	7	3	0.4	0–2	3	8	4.1	3–6
70–	5	3	0.4	0–1	3	5	3.9	2–6

TABLE 2

Age and fertility categories for females, showing the number of single and married individuals in each of the 22 categories, the mean fertility per individual and the range of fertility per individual for each category.

	BELOW AVERAGE FERTILITY				ABOVE AVERAGE FERTILITY			
	NUMBER OF INDIVIDUALS		MEAN FER-TILITY	RANGE OF FER-TILITY	NUMBER OF INDIVIDUALS		MEAN FER-TILITY	RANGE OF FER-TILITY
AGE	SINGLE	MARRIED			SINGLE	MARRIED		
20–24	14	19	0.0		1	18	1.4	1–2
25–29	12	21	0.0		1	29	1.6	1–3
30–34	4	7	0.5	0–1	0	20	2.7	2–4
35–39	7	5	0.3	0–1	1	12	3.2	2–6
40–44	4	6	0.0		1	13	2.1	1–4
45–49	8	13	0.5	0–1	1	22	2.7	2–5
50–54	2	6	0.6	0–1	1	15	2.9	2–7
55–59	2	4	0.7	0–2	2	5	4.0	3–8
60–64	4	3	1.0	0–2	2	3	4.6	3–6
65–69	6	2	1.5	0–2	8	1	5.1	3–12
70–	10	1	0.9	0–2	6	2	4.0	3–7

less than the median for its sex and age group. In total, therefore, the males were classified into 18 and the females into 22 categories. These categories, together with the mean and the range of the number of children per individual and the number of single and married individuals in each, are shown for males and females in tables 1 and 2, respectively. It may be seen from these tables that the largest number of children reported by any male was 6 and that the most fertile female produced 12 children. The lowest number of children produced was zero for both sexes. Taken as a whole, the sample can scarcely be regarded as a highly fertile one.

MEASUREMENTS

All anthropometric measurements were made in the anthropometry room of the University of Michigan's Institute of Human Biology. Measurements were taken on the subject with shoes and clothing external to shirts and blouses removed. The sleeves of shirts or blouses were rolled up in order to make the various arm measurements, and pant legs or skirts were elevated in order to make measurements on the lower leg.

The measurements were made with an anthropometer, sliding caliper, spreading caliper, Hrdlička caliper, steel metric tape, and with platform scales calibrated in quarter pounds. Wall-mounted scales of standard metric sticks were used to measure stature and span. A low bench with a vertical scale was used to measure sitting height.

The methods of taking the measurements are those of Martin (1928). The measurements used in this aspect of the study, together with the instruments employed and the numbers by which Martin designated the measurements, are given below.

MEASUREMENTS OF BODY AND EXTREMITIES

Net weight. Platform scales. Gross weight (Martin 71) minus estimated weight of clothes.

Stature. Wall scale (Martin 1).

Cervical height. Anthropometer. Height from the floor of the apex of the tip of the spinous process of the seventh cervical vertebra.

Span. Wall scale (Martin 17).

Sitting height. Sitting height scale (Martin 23).

Biacromial breadth. Anthropometer used as a sliding caliper (Martin 35).

Forearm length. Anthropometer used as a sliding caliper (Martin 48).

Hand length. Sliding caliper (Martin 49).

Middle finger length. Sliding caliper (Martin 51).
Hand breadth. Sliding caliper (Martin 52).
Foot length. Anthropometer used as a sliding caliper (Martin 58).
Minimum neck circumference. Steel tape, just below thyroid cartilage (Martin 63).
Maximum midarm circumference. Steel tape (Martin 65).
Maximum forearm circumference. Steel tape (Martin 66).
Minimum wrist circumference. Steel tape (Martin 67).
Maximum calf circumference. Steel tape (Martin 69).
Minimum ankle circumference. Steel tape (Martin 70).
Bicondylar breadth of elbow. Hrdlička caliper.
Bicondylar breadth of knee. Hrdlička caliper.

MEASUREMENTS OF HEAD AND FACE

Head length. Spreading caliper (Martin 1).
Head breadth. Spreading caliper (Martin 3).
Head height. Anthropometer used as a sliding caliper (Martin 15).
Minimum frontal breadth. Spreading caliper (Martin 4).
Bizygomatic breadth. Spreading caliper (Martin 6).
Bigonial breadth. Spreading caliper Martin 8).
Bipalpebral breadth. Sliding caliper (Martin 10).
Interpupillary distance. Sliding caliper (Martin 12).
Nose height. Sliding caliper (Martin 21).
Nose breadth. Sliding caliper (Martin 13).
Total facial height. Sliding caliper (Martin 18).
Ear height. Sliding caliper (Martin 29).
Ear lobe height. Sliding caliper (Martin 33).

In addition to the direct measurements listed above, the following ratios were also investigated:

$$\frac{\text{stature}}{\text{span}},\ \frac{\text{sitting height}}{\text{stature}},\ \frac{\text{shoulder breadth}}{\text{stature}},\ \frac{\text{hand breadth}}{\text{hand length}},$$

$$\frac{\text{head breadth}}{\text{head length}},\ \frac{\text{bizygomatic breadth}}{\text{head breadth}},\ \frac{\text{bigonal breadth}}{\text{minimum frontal breadth}},$$

$$\frac{\text{nose height}}{\text{total facial height}},\ \text{and}\ \frac{\text{nose breadth}}{\text{nose height}}.$$

Most of the measurements were performed by the authors. Measurements were also taken by Neil Tappen, Van Harris, and Jack Kelso.

ANALYSES

The relationships between the anthropometric traits and fertility were analyzed separately for each trait and each sex by a series of $r \times 2$ analyses of variance, there being r age groups (with $r = 9$ for males; $r = 11$ for females) and two fertility classes (namely, above average and below average fertility) for each analysis. Since 41 characters were investigated, this means that 82 separate analyses of variance were performed. The method of analysis is that described by Snedecor (1956, pp. 382-384) for disproportionate subclass numbers.

Although many of the anthropometric traits are conspicuously correlated with age, the investigation of these relationships is not within the scope of this paper. Individuals were classified into age groups only in order to prevent the relationships with age from being confounded with those between the anthropometric traits and fertility, the latter relationships being the only ones under consideration in this paper.

RESULTS

A summary of the results of the analyses is given in tables 3 and 4 for males and females, respectively. These tables include for each trait an estimate, D, of the difference in the mean value of the trait between the above average and the below average fertility groups (*i. e.*, above average minus below average fertility), the F-value for the relation between the trait and fertility, the F-value for the effect on the trait of the interaction between age and fertility, and the probabilities of obtaining F-values as large as those observed on the assumption that the anthropometric traits and fertility are independent of one another. In discussing the various traits a single asterisk will indicate significance at the 5% level; two asterisks will indicate significance at the 1% level.

Examination of table 3 reveals that in the males the F-values for fertility were significant for the following characters: Head length*, bizygomatic breadth*, minimum neck circumference**, weight**, biacromial breadth*, maximum forearm circumference*, maximum midarm circumference**, minimum wrist circumference**, maximum calf circumference**, bicondylar breadth of knee* and $\frac{\text{stature}^*}{\text{span}}$.

The first 10 of these characters involve direct measurements and were positively correlated with fertility. In other words, the character under question had a larger mean value in the more fertile than in the

TABLE 3. MALES

Results of Analyses of Variance for each of the 41 traits, showing an estimate, D, of the difference in the mean value of each trait between the above average and the below average fertility groups (i. e., above average fertility minus below average fertility), the F-value for the relationship between the trait and fertility, the F-value for the interaction between age and fertility, and the probabilities of obtaining F-values as large as those observed on the assumption that the anthropometric traits and fertility are independent of one another. The F for fertility contains 1 degree of freedom in the numerator and 212 in the denominator; that for interaction has 8 degrees of freedom in the numerator and 212 in the denominator.

CHARACTER	D	F-VALUE FOR FERTILITY	PROBABILITY OF LARGER F	F-VALUE FOR INTERACTION	PROBABILITY OF LARGER F
Weight	11.33 lb	10.30	$.005 > P > .001$	2.02	$.050 > P > .025$
Stature	—3.66 mm	0.18	$.75 > P > .50$	2.92	$.005 > P > .001$
Cervical height	0.89 mm	0.01	$.95 > P > .90$	0.78	$.75 > P > .50$
Span	13.92 mm	2.02	$.25 > P > .10$	1.01	$.50 > P > .30$
Sitting height	5.26 mm	1.48	$.25 > P > .10$	0.85	$.75 > P > .50$
Biacromial breadth	5.53 mm	3.97	$.050 > P > .025$	0.98	$.50 > P > .30$
Forearm length	—0.11 mm	0.004	$.975 > P > .950$	1.25	$.30 > P > .10$
Hand length	2.49 mm	1.50	$.25 > P > .10$	1.20	$.50 > P > .30$
Middle finger length	2.39 mm	2.95	$.10 > P > .05$	0.40	$.95 > P > .90$
Hand breadth	1.02 mm	3.11	$.10 > P > .05$	1.12	$.50 > P > .30$
Foot length	0.27 mm	0.03	$.90 > P > .75$	0.71	$.75 > P > .50$
Minimum neck circumference	9.90 mm	10.09	$.005 > P > .001$	1.52	$.25 > P > .10$
Maximum midarm circumference	13.44 mm	13.95	$.0005 > P$	2.77	$.010 > P > .005$
Maximum forearm circumference	5.67 mm	4.30	$.050 > P > .025$	2.63	$.010 > P > .005$
Minimum wrist circumference	4.37 mm	9.74	$.005 > P > .001$	1.91	$.10 > P > .05$
Maximum calf circumference	10.72 mm	8.73	$.005 > P > .001$	2.55	$.025 > P > .010$
Minimum ankle circumference	4.18 mm	2.77	$.10 > P > .05$	0.94	$.50 > P > .30$
Bicondylar breadth of elbow	1.56 mm	3.01	$.10 > P > .05$	1.59	$.30 > P > .10$
Bicondylar breadth of knee	1.48 mm	4.06	$.050 > P > .025$	1.34	$.25 > P > .10$
Head length	2.33 mm	6.11	$.025 > P > .010$	0.93	$.50 > P > .30$
Head breadth	0.83 mm	1.43	$.25 > P > .10$	1.21	$.30 > P > .10$
Head height	1.23 mm	1.52	$.25 > P > .10$	1.26	$.30 > P > .10$
Minimum frontal breadth	1.25 mm	3.44	$.10 > P > .05$	0.47	$.90 > P > .75$
Bizygomatic breadth	1.48 mm	4.33	$.050 > P > .025$	1.34	$.25 > P > .10$
Bigonial breadth	1.50 mm	3.47	$.10 > P > .05$	1.55	$.25 > P > .10$
Bipalpebral breadth	0.48 mm	0.66	$.50 > P > .25$	0.49	$.90 > P > .75$

TABLE 3. MALES (Continued)

CHARACTER	D	F-VALUE FOR FERTILITY	PROBABILITY OF LARGER F	F-VALUE FOR INTERACTION	PROBABILITY OF LARGER F
Interpupillary distance	0.23 mm	0.29	$.75 > P > .50$	1.27	$.30 > P > .10$
Nose height	0.36 mm	0.43	$.75 > P > .50$	1.76	$.10 > P > .05$
Nose breadth	−0.078 mm	0.04	$.90 > P > .75$	0.92	$.75 > P > .50$
Total facial height	−0.003 mm	0.001	$.975 > P > .950$	1.07	$.50 > P > .30$
Ear height	1.06 mm	3.67	$.10 > P > .05$	1.62	$.25 > P > .10$
Ear lobe height	0.13 mm	0.14	$.75 > P > .50$	1.75	$.10 > P > .05$
$\frac{\text{Stature}}{\text{Span}}$	−0.0054	4.00	$.050 > P > .025$	1.04	$.50 > P > .30$
$\frac{\text{Sitting height}}{\text{Stature}}$	0.0020	1.63	$.25 > P > .10$	0.42	$.95 > P > .90$
$\frac{\text{Shoulder breadth}}{\text{Stature}}$	0.0028	3.44	$.10 > P > .05$	1.78	$.10 > P > .05$
$\frac{\text{Hand breadth}}{\text{Hand length}}$	0.0071	2.10	$.25 > P > .10$	0.82	$.75 > P > .50$
$\frac{\text{Head breadth}}{\text{Head length}}$	−0.0052	1.14	$.50 > P > .25$	1.41	$.25 > P > .10$
$\frac{\text{Bizygomatic breadth}}{\text{Head breadth}}$	0.0049	1.53	$.25 > P > .10$	0.92	$.75 > P > .50$
$\frac{\text{Bigonial breadth}}{\text{Minimum frontal breadth}}$	0.0011	0.016	$.95 > P > .90$	1.31	$.25 > P > .10$
$\frac{\text{Nose height}}{\text{Total facial height}}$	0.0027	0.55	$.50 > P > .25$	2.40	$.025 > P > .010$
$\frac{\text{Nose breadth}}{\text{Nose height}}$	−0.0051	0.26	$.75 > P > .50$	1.80	$.10 > P > .05$

less fertile group. The last character, however, namely stature divided by span, was negatively correlated with fertility. Considering the entire group of 41 characters it will be seen that for males 28 of 32 measurements and 6 of the 9 ratios were positively related with fertility, although only for the characters previously listed did the F-values for fertility reach the 5% level of significance.

The F-values for the interaction between age and fertility were significant, among the males, for the following characters: Weight*, stature**, maximum forearm circumference**, maximum midarm circumference**, maximum calf circumference* and $\frac{\text{nose height*}}{\text{total facial height}}$.

TABLE 4. FEMALES

Results of Analyses of Variance for each of the 41 traits, showing an estimate, D, of the difference in the mean value of each trait between the above average and the below average fertility groups (i. e., above average fertility minus below average fertility), the F-value for the relationship between the trait and fertility, the F-value for the interaction between age and fertility, and the probabilities of obtaining F-values as large as those observed on the assumption that the anthropometric traits and fertility are independent of one another. The F for fertility contains 1 degree of freedom in the numerator and 302 in the denominator; that for interaction contains 10 degrees of freedom in the numerator and 302 in the denominator.

CHARACTER	D	F-VALUE FOR FERTILITY	PROBABILITY OF LARGER F	F-VALUE FOR INTERACTION	PROBABILITY OF LARGER F
Weight	4.33 lb	2.51	$.25 > P > .10$	0.54	$.90 > P > .75$
Stature	—10.15 mm	2.14	$.25 > P > .10$	0.20	$.999 > P > .995$
Cervical height	—11.07 mm	2.93	$.10 > P > .05$	0.41	$.95 > P > .90$
Span	—3.38 mm	0.17	$.75 > P > .50$	0.59	$.90 > P > .75$
Sitting height	—7.54 mm	4.31	$.050 > P > .025$	1.03	$.50 > P > .30$
Biacromial breadth	0.70 mm	0.13	$.75 > P > .50$	0.51	$.90 > P > .75$
Forearm length	—0.28 mm	0.04	$.90 > P > .75$	0.93	$.75 > P > .50$
Hand length	0.24 mm	0.06	$.90 > P > .75$	1.02	$.50 > P > .30$
Middle finger length	0.53 mm	0.30	$.75 > P > .50$	1.37	$.25 > P > .10$
Hand breadth	0.98 mm	4.64	$.050 > P > .025$	0.84	$.75 > P > .50$
Foot length	0.14 mm	0.01	$.95 > P > .90$	0.48	$.95 > P > .90$
Minimum neck circumference	2.93 mm	1.68	$.25 > P > .10$	0.99	$.50 > P > .30$
Maximum midarm circumference	9.66 mm	6.46	$.025 > P > .010$	0.82	$.75 > P > .50$
Maximum forearm circumference	5.95 mm	7.81	$.010 > P > .005$	1.17	$.50 > P > .30$
Minimum wrist circumference	2.20 mm	4.06	$.050 > P > .025$	1.06	$.50 > P > .30$
Maximum calf circumference	4.34 mm	1.36	$.25 > P > .10$	1.45	$.25 > P > .10$
Minimum ankle circumference	3.85 mm	2.30	$.25 > P > .10$	0.87	$.75 > P > .50$
Bicondylar breadth, arm	1.11 mm	1.44	$.25 > P > .10$	0.40	$.95 > P > .90$
Bicondylar breadth, leg	0.79 mm	1.35	$.25 > P > .10$	0.84	$.75 > P > .50$
Head length	—.25 mm	0.12	$.75 > P > .50$	0.35	$.975 > P > .950$
Head breadth	—.18 mm	0.11	$.75 > P > .50$	0.67	$.90 > P > .75$
Head height	1.30 mm	2.45	$.25 > P > .10$	1.18	$.50 > P > .30$
Minimum frontal breadth	—0.55 mm	0.99	$.50 > P > .25$	1.48	$.25 > P > .10$
Bizygomatic breadth	0.32 mm	0.35	$.75 > P > .50$	0.46	$.95 > P > .90$
Bigonial breadth	—0.46 mm	0.63	$.50 > P > .25$	0.54	$.90 > P > .75$
Bipalpebral breadth	—0.21 mm	0.22	$.75 > P > .50$	1.00	$.50 > P > .30$

TABLE 4. FEMALES (Continued)

CHARACTER	D	F-VALUE FOR FERTILITY	PROBABILITY OF LARGER F	F-VALUE FOR INTERACTION	PROBABILITY OF LARGER F
Interpupillary distance	—0.53 mm	1.85	$.25>P>.10$	0.76	$.75>P>.50$
Nose height	—1.02 mm	7.18	$.010>P>.005$	0.76	$.75>P>.50$
Nose breadth	0.17 mm	0.39	$.75>P>.50$	1.19	$.30>P>.10$
Total facial height	—0.58 mm	0.71	$.50>P>.25$	1.25	$.25>P>.10$
Ear height	0.06 mm	0.02	$.90>P>.75$	0.92	$.75>P>.50$
Ear lobe height	—0.25 mm	0.55	$.50>P>.25$	2.18	$.025>P>.010$
$\frac{\text{Stature}}{\text{Span}}$	—0.0048	3.62	$.10>P>.05$	1.57	$.25>P>.10$
$\frac{\text{Sitting height}}{\text{Stature}}$	—.0010	0.38	$.75>P>.50$	0.86	$.75>P>.50$
$\frac{\text{Shoulder breadth}}{\text{Stature}}$	.0018	2.58	$.25>P>.10$	0.56	$.90>P>.75$
$\frac{\text{Hand breadth}}{\text{Hand length}}$	.0049	4.21	$.050>P>.025$	0.76	$.75>P>.50$
$\frac{\text{Head breadth}}{\text{Head length}}$	.0000	.00012	$.975>P>.950$	0.65	$.90>P>.75$
$\frac{\text{Bizygomatic breadth}}{\text{Head breadth}}$	.0031	1.03	$.50>P>.25$	1.27	$.30>P>.10$
$\frac{\text{Bigonial breadth}}{\text{Minimum frontal breadth}}$	.0006	0.01	$.95>P>.90$	1.29	$.25>P>.10$
$\frac{\text{Nose height}}{\text{Total facial height}}$	—.0086	6.96	$.010>P>.005$	0.58	$.90>P>.75$
$\frac{\text{Nose breadth}}{\text{Nose height}}$	.0156	4.26	$.050>P>.025$	1.35	$.25>P>.10$

For 4 of these characters, namely weight, maximum forearm circumference, maximum midarm circumference, and maximum calf circumference, the F-values for fertility were also significant. However, even though the main effect of fertility was not significant for stature or for $\frac{\text{nose height}}{\text{total facial height}}$, the existence for these traits of an interaction between age and fertility would be indicative of a relationship between the traits and fertility, although the nature of the relationship would vary from age group to age group.

From table 4 it may be seen that in the females the F-values for fertility are significant for the following characters: Nose height**, sitting height*, maximum forearm circumference**, maximum midarm circumference*, minimum wrist circumference*, hand breadth*,

$$\frac{\text{hand breadth*}}{\text{hand length}}, \frac{\text{nose breadth*}}{\text{nose height}}, \text{ and } \frac{\text{nose height**}}{\text{total facial height}}.$$

The first 6 of these characters involve direct measurements, and the last three are ratios of certain measurements. Of the direct measurements, the first two in the list, namely nose height and sitting height, were negatively correlated with fertility, and the last 4, namely maximum forearm circumference, maximum midarm circumference, minimum wrist circumference, and hand breadth, were positively correlated with fertility. In other words, the mean values for nose height and sitting height were larger in the less fertile than in the more fertile group, and the mean values for maximum forearm circumference, maximum midarm circumference, minimum wrist circumference, and hand breadth were larger for the more fertile than for the less fertile group. Of the three ratios for which the *F*-values for fertility were significant in the females, the last one, namely, $\frac{\text{nose height}}{\text{total facial height}}$, was negatively correlated with fertility, whereas $\frac{\text{hand breadth}}{\text{hand length}}$ and $\frac{\text{nose breadth}}{\text{nose height}}$ were positively correlated with fertility. Considering the entire group of 41 characters it may be seen that for females 18 of the 32 measurements and 6 of the 9 ratios were positively correlated with fertility, although only for the characters previously listed did the *F*-values for fertility reach the 5% level of significance.

The *F*-value for the interaction between age and fertility was significant at the 5% level, in the females, only for ear lobe height.

For the three characters maximum midarm circumferences, maximum forearm circumference, and minimum wrist circumference, the *F*-values for fertility were significant for both males and females; the character $\frac{\text{nose height}}{\text{total facial height}}$ gave a significant *F* for fertility in the females and a significant *F* for interaction between age and fertility in the males.

DISCUSSION

The first question to be considered is whether the data and analyses here described afford substantial evidence for the existence of a relationship between fertility and body dimensions. Since 41 separate analyses were performed on each sex—a total of 82 analyses—it might be expected that for some of the characters the *F*-values would approach the 5% level of significance even if there were no relationship between the characters and fertility. In fact, if all of the traits were independent of one another, the *F*-values should reach the 5% level of significance for about 5% of the analyses. Actually, many of the traits are highly correlated with one another, and it is therefore impossible rigorously to consider the question without obtaining all of the cross products among the traits. Nevertheless, it is the view of the authors that the number of significant *F*-values far exceeds the number likely to be obtained by chance, even allowing for correlations among the variables. It does not appear likely that chance alone is responsible for the fact that among the males 28 of the 32 direct measurements have a higher mean value in the above average than in the below average fertility group. Among the females, on the other hand, where only 18 of the 32 direct measurements have a higher mean value in the more fertile than in the less fertile group, chance could be more reasonably invoked. But even among the females the number of differences which exceed the 5% level of significance is greater than would be expected by chance alone. The authors conclude, therefore, that there exists a real difference in certain body measurements between the two fertility groups of both sexes.

The above conclusion is strengthened by the fact that the observed differences fit a definite pattern. For both males and females the measures of body breadth and thickness, namely, weight, biacromial breadth, maximum forearm circumference, maximum midarm circumference, hand breadth, bicondylar breadth of arm, minimum wrist circumference, bicondylar breadth of leg, maximum calf circumference, and minimum ankle circumference, were in every case greater in the more fertile than in the less fertile group. Moreover, the probabilities for the *F*-values associated with these characters and fertility were in every case other than biacromial breadth of females lower than 0.25.

On the other hand, for the measures of length of the body and extremities, namely, stature, cervical height, sitting height, span, forearm length, hand length, middle finger length, and foot length, a rather different situation is evident. Among the females the mean value of the less fertile group exceeded that for the more fertile group for all of the above traits other than hand length, middle finger length, and foot length. However, the probabilities associated with the *F*-values relating these traits with fertility are less than 0.25 only for stature, sitting height, and cervical height; they are greater than 0.50 for span, forearm length, hand length, middle finger length, and foot length. Among the males, the mean value of the more fertile group exceeded that of the less fertile group for all of the above mentioned 8 measures of length except stature and forearm length. But for half of these traits the probabilties for the F-values relating the trait with fertility exceed 0.50, so that collectively there is little evidence in the males for either a consistent positive or negative relationship between fertility and these various measures of length. It should be noted, however, that for stature in males the interaction between age and fertility was significant at the 1% level. This suggests that there may actually be a relationship between stature and fertility in the males, even though the nature of the relationship varies from age group to age group.

Taken as a whole the data suggest that for the population sampled in this study individuals of both sexes who are above average in fertility tend to be more stoutly built than individuals of below average fertility, that females who are above average in fertility tend to be shorter than females who are below average in fertility, and that there is little consistent relationship between fertility and the various measures of length in the males.

Definite trends are also to be found for the measurements of the face and head. The 6 measures: head length, head breadth, head height, minimum frontal breadth, bizygomatic breadth, and bigonial breadth, are all correlated with general head size. For all of these traits the mean value of the more fertile males exceeded that for the less fertile males. And the probabilities associated with the F-values relating these traits with fertility are in every case less than 0.25. It appears that the males of the Ann Arbor population who are above average in fertility tend to have larger heads than those who are below average in fertility. No such trend is evident in the females. However, there is a relationship among the females between fertility and both the absolute and relative length of the nose, the characters nose height and $\frac{\text{nose height}}{\text{total facial height}}$ tending to be smaller and $\frac{\text{nose breadth}}{\text{nose height}}$ larger in the above average than in the below average fertility groups. These various head and facial characters seem to be involved in popular ideas of masculine and feminine beauty, and it is possible that the relationships between these measurements and fertility are at least in part the result of sexual selection.

There is undoubtedly a relationship between the anthropometric measurements considered in this study and the somatotypes of W. H.

Sheldon. The data appear to indicate that the more fertile individuals of the Ann Arbor population tend to be lower in ectomorphy than are the less fertile individuals. However, our data do not enable us to distinguish accurately enough between endomorphy and mesomorphy to compare the relationship with fertility of these two components of body structure.

The interpretation of the interactions between age and fertility warrants some discussion since the existence, for a given trait, of such an interaction suggests that the relationship between that trait and fertility has not been constant over the time covered by the data. There is some tendency for the differences, D (above average fertility minus below average fertility), between the two fertility groups to be different in the 35–49 year old age groups from those of the other age categories. This is particularly noticeable in those traits for which the interaction between age and fertility is significant. Thus, for both weight and maximum forearm circumference in males these differences are negative for the 35–39, 40–44, and 45–49 year old age groups and positive for all other age groups. For maximum midarm circumference of males the difference is negative only for the 40–44 year old group, and for maximum calf circumference of males it is negative only for the 40–44 and 45–49 year old groups. Since these ages are as of 1952-54 the individuals in the 35–49 year old categories would have been between 14 and 30 years of age in 1933, and it is clear that the most vigorous reproductive years of many of the individuals in these age groups coincided at least in part with the economic depression of that period. It is possible that during this depression the usual positive correlation between fertility and stockiness of build in males was temporarily reversed. For the other traits with a significant F-value for the interaction between age and fertility, namely stature and $\frac{\text{nose height}}{\text{total facial height}}$ of males and ear lobe height of females, meaningful patterns are less evident.

It is unlikely, although perhaps possible, that the relationship between fertility and body type observed in this study is due to the inclusion in the sample of members of racial and cultural isolates which differ both in physical characteristics and fertility. The city of Ann Arbor contains relatively few isolates, as compared with larger cities, and only individuals of European descent were included in the sample. Moreover, the geographic stratification of the sampling would tend to prevent any such isolates from exerting an undue effect on the sample. The data appear to provide substantial evidence for the existence, in the Ann Arbor population, of a real relationship between physical type and fertility.

The evolutionary and genetic implications of these observations depend on the underlying causal connections. The relationship, in both sexes, between fertility and stoutness may well indicate that stockiness of build tends to increase the fertility of an individual. And this could mean that the genetic structure of the population is changing—that the population is evolving in the direction of greater stoutness. On the other hand, since much of the differentiation with reference to stoutness occurs in middle age, it is possible that the observed relationship between fertility and body build is caused by factors such as marital status or socio-economic level independently influencing both body build and fertility, and in this event these relationships would not be of particular evolutionary significance. The question, then, is whether a stocky build in some way results in greater fertility or whether above average fertility—or the factors associated with it—tends to result in a stocky build. There is, of course, no reason to assume that these two alternatives are mutually exclusive. To assess their relative importance, however, it will be necessary to obtain measurements on young adults prior to differentiation with reference to such factors as marital status and socio-economic level and then to correlate these measurements with subsequent reproductive performance. Such a study is now in progress.

The correlation between fertility and size of head in males or between fertility and nose length and body height in females can scarcely be attributed to an effect on these characters of marital status or of socio-economic level, inasmuch as most of the differentiation with reference to these traits occurs prior to marriage. It is known that variation in these traits is largely under genetic control, and hence the observed correlations with fertility would appear to indicate that natural or sexual selection is operative on them.

SUMMARY

The analyses herein described show that for both males and females of the Ann Arbor population there is a tendency for individuals of above average fertility to be more stockily built than individuals of less than average fertility. However, it is unclear from these analyses whether a stocky build tends to result in above average fertility or whether the factors associated with above average fertility—such as marital status and socio-economic level—tend to result in a stocky build. Consequently, the genetic and evolutionary implications of the correlations are in doubt.

The analyses also suggest that the more fertile males tend to have larger heads than the less fertile males and that the more fertile females tend to be shorter in stature and have smaller noses than the less fertile females. It seems likely that these relationships are due to the operation on these traits of either sexual selection or natural selection, or both, and that they are therefore of appreciable genetic and evolutionary importance.

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STUDIES ON THE OCCIPITAL BONE IN AFRICA

IV. COMPONENTS AND CORRELATIONS OF OCCIPITAL CURVATURE IN RELATION TO CRANIAL GROWTH

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IN previous studies in this series, it has been demonstrated that the occipital curvature in two African physical types, Bushman and Negro, falls into two clear-cut phenotypic classes (Tobias 1958a, 1958b). Bushmen have *curvoccipital* and Negroes *planoccipital* crania. It was further demonstrated that, up to a dental age of about six years, both groups have well-curved occipitals, a recognized infantile feature. Thereafter, a progressive flattening sets in, in Negro crania, whereas Bush crania retain the infantile contour to adulthood (Tobias 1958c).

The existence of two such well-defined categories of occipital morphology has thus led to a study of the developmental processes which engendered them. Once such processes have been clarified, the way is opened to an understanding of the genetical bases of occipital curvature, for, generally speaking, genes operate, not on phenotypic entities, but on processes, altering their rate, timing or direction.

It is the purpose of this paper to enquire whether, in terms of the developmental processes leading to the adult occipital curvature, a particular occipital index occurring in two crania may be assumed to connote identity of pattern or process. The question is posed because of the facile assumption, often made in biometrical physical anthropology, that a metrical resemblance necessarily connotes a genetic affinity. This assumption has been questioned recently by le Gros Clark (1955) and Mednick and Washburn (1956), both of whom have pointed out that metrical and statistical comparisons are often made between biological incomparables.

In seeking a biological understanding of what the measurements reveal, I have been guided by the principle that measurements and indices are simply a means of expressing some morphological features more precisely: measurements are no substitute for morphology. In this study, not only has a biometrical analysis been made of the curvature indices of the occipital bone; but an attempt has been made to dissect the biological significance behind the bare measurements.

MATERIAL AND METHODS

The protocols of the African cranial series utilized in these investigations have been set forth fully in the first paper of this series (Tobias 1958a).

Occipital curvature has been assessed by two indices, both based upon the chord ($S'3$) and median sagittal arc ($S3$) between lambda and opisthion. The simpler Chord-Arc Index is derived by expressing $S'3$ as a percentage of $S3$. Pearson's Occipital Index (Oc. I.) expresses the percentage ratio of the radius of curvature of the occipital bone to the chord $S'3$, assuming that the arc $S3$ is an even curve. The following formula has been devised for Pearson's Oc. I.:

$$\text{Oc. I.} = \frac{S3}{S'3}\sqrt{\frac{S3}{24(S3 - S'3)}}.$$

Values for both indices are higher in crania with flat occipitals, and lower in crania with well-curved occipitals.

A full discussion of the history of the two indices is given in the first paper of the series (1958a), whilst the relative usefulness of the two indices will be considered in the last (1958d).

An answer to the queries posed in the introduction to this paper has been sought in several ways: by analysing measureable components of the occipital arc and chord and by directly comparing different skulls with different indices. Thereafter, the correlation between occipital curvature and other metrical features of the cranium is considered and the deduced morphogenetic changes of the occiput are analysed in terms of modern concepts of cranial growth.

An obvious median point dividing the occipital arc is the inion. It has the advantage that it corresponds in position to the internal occipital protuberance, to which attaches the tentorium cerebelli, separating the cerebrum above from the cerebellum below. The identification of inion may, however, occasion considerable difficulty. In this study the point was taken as the meeting-place of the *supreme* nuchal lines and this generally coincided with the common horizontal tangent to the two *superior* nuchal lines. In assessing the point, the writer was especially guided by a discussion on the subject by Tildesley (1920-21, pp. 184-5).

The position of inion having been determined, I measured the chords and arcs of the occipital bone above and below inion. From these measurements, two simple chord-arc indices were derived (the Occipital Cerebral Index and the Occipital Cerebellar Index of Sauter, 1941-46). Further, using a Hjortsjo Trigonometer, I computed the angle subtended by the lambda-opisthion chord at inion, from a knowledge of the three chords, lambda-inion, inion-opisthion and lambda-opisthion. These operations were carried out on 55 adult male Somali crania in the Duckworth Laboratory, Cambridge University, and on the Bushman and Hottentot crania in the Anatomy Departments at Cambridge and Edinburgh Universities. For comparison, there were available data on Bushmen, Hottentots, Griquas and Swiss (Sauter 1941-46).

Components of the Occipital Curvature.

The mean values of the Occipital Cerebral and Occipital Cerebellar

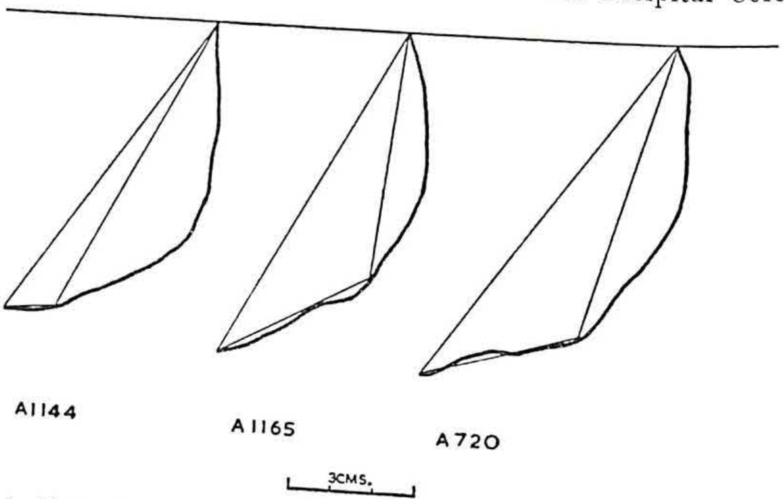

Fig. 1. The Varying Components of the Occipital Curve. In Each Craniogram the Lambda-Inion and Inion-Opisthion Chords have been Indicated. A 1165 and A 720 have Identical Indices and A 1144 a Very Similar One.

A 1144: Immature Bush type with occipital indices 82.8 and 59.42.
A 1165: Bush type with occipital indices 83.5 and 60.15.
A 720: Xosa with occipital indices 83.5 and 60.15.

Indices, and of the lambda-inion-opisthion angle for four African cranial series and one European series are given in table 1.

In all series, the cerebral curvature is more marked than the cerebellar, except that in the Swiss female series, the cerebellar curvature is slightly greater. The Swiss and Somalis have flatter cerebral curvatures than the Bush, Hottentot and Griqua.

The possibility that the total occipital curvature depended in part on that of the component segments was tested by calculating correlation coefficients (r) between the occipital indices and the indices of the component parts of the occipital bone. The values recorded for these *intra-racial* correlations are tabulated below, along with correlation coefficients between the occipital indices and other cranial characters, given by Pearson and Davin (1924) and by Wagner (1937). The italicized values in table 2 indicate a definite correlation, whether positive or negative.

TABLE 1

SERIES	SEX	NUMBER	OC. CEREBRAL I.	OC. CEREBELLAR I.	INION ANGLE
Bushman	M	58	93.7	97.69	125.1°
	F	43	94.0	98.06	120.5°
Griqua	M	14	93.0	94.48	120.8°
	F	2	93.7	100.00	117.2°
Hottentot	M	19	93.0	98.93	118.6°
	F	4	92.8	?	123.5°
Somali	M	55	94.35	98.07	125.1°
Swiss	M	50	96.2	96.46	119.0°
	F	50	96.3	95.22	122.7°

Considering firstly the occipital bone itself, we see that, as was to be expected, both occipital indices ($100S'3/S3$ and Oc. I.) are strongly positively correlated with the angle at inion: a *strong* curvature, in other words, may be expressed as a *low* index or a more *acute* angle, while a *flat* occipital curve is conveyed by a *high* index or a more *obtuse* angle. In exactly the same way, Wagner (1937) demonstrated a very high correlation between Pearson's Oc. I. and the simple chord-arc index (*vide* the first three entries in table 2) . No such high correlation exists, however, with the curvature above or below the inion, as determined by the lambda-inion chord-arc index (occipital cerebral index) and the inion-opisthion chord-arc index (occipital cerebellar index) respectively. There is virtually no correlation with the curvature of lambda-inion, but there is a small though definite positive correlation (.201, .213) with the curvature between inion and opisthion. Up to a point, the more curved the cerebellar part of the occipital arc is, the more curved will the occipital arc as a whole be.

TABLE 2

Correlation coefficients between occipital indices and other cranial characters

	100 S'3/S3	Oc. I.	Series & Sex	Reference
Pearson's Oc. I.	+*.963*	—	Australian♂	Wagner, 1937
	+*.873*	—	Sandwich Is.♂	"
	+*.868*	—	Maori♂	"
Oc. Cerebral Index	+.082	+.071	Somali♂	This study
Oc. Cerebellar Index	+*.201*	+*.213*	Somali♂	This study
Lambda-inion-opisthion Angle	+*.808*	+*.833*	Somali♂	This study
Cranial capacity	—	—.0547	XXVI-XXXth Dyn. Egyptians♂	Pearson and
	—	—.1044	" ♀	Davin, 1924
Max. Cranial Length	—	—*.3236*	" ♂	"
	—	—*.3720*	" ♀	"
Max. Cranial (Bi-parietal) Breadth	—	—.0882	" ♂	"
	—	—.1033	" ♀	"
Auricular Height	—	+.0577	" ♂	"
	—	+.0881	" ♀	"
Basion-Nasion Length	—	+.0113	" ♂	"
	—	—.0118	" ♀	"
Sagittal (Nasion-Opisthion) Arc	—	—*.2257*	" ♂	"
	—	—*.2218*	" ♀	"
Parietal chord-arc Index	—*.252*	—	Australian♂	Wagner, 1937
	—*.312*	—	Sandwich Is.♂	"
	—*.221*	—	Maori♂	"
Frontal chord-arc Index	+*.157*	—	Australian♂	"
	+.092	—	Sandwich Is.♂	"
	+*.317*	—	Maori♂	"
Breadth-Length Index	+*.262*	+*.295*	Australian♂	"
	+*.464*	+*.553*	Sandwich Is.♂	"
	+*.137*	+*.207*	Maori♂	"
Profile Angle (between standard horizontal plane and nasio-alveolar length)	—	—.0416	Egyptian♂	Pearson and
	—	—.1153	" ♀	Davin, 1924
Foramen magnum Index	—	—.0317	" ♂	"
	—	—.0311	" ♀	"

This particular correlation is an example of a correlation between two indices, of which a component of one ($S3$) "covers" a component of the other (inion-opisthion arc). Pearson and Davin (1924) showed that correlation is obviously high between "covering" measurements, such as the sagittal arc and the lambda-opisthion arc ($S3$) ($r = .5273$)

or the horizontal circumference and maximal cranial length ($r = .8834$). As they put it (p. 349)

> The "common cause groups" in all these cases are so obvious that we can state *a priori* that there will be high correlations. They are not true organic correlations, but to a large extent result from what we may term mathematical necessity, i. e. from the compound character of the anthropometric measurements.

When *indices* are considered, correlations are not significant unless two components "cover" each other in whole or in part, according to Pearson and Davin. Thus, there is a correlation between Nasal and Upper Facial Indices of —.4607 (♂) and —.4902 (♀): for Upper Facial Height "covers" Nasal Height and Facial Breadth "covers" Nasal Breadth. Where only one component of the first index "covers" one component of the second, the correlation is smaller: for example, the Palatal and Upper Facial Indices are correlated to the extent of —.1325 (♂) and —.1743 (♀), since Facial Breadth "covers" Palatal Breadth. In other words, the correlation between indices comprising "covering" components increases with the extent of the "covering"; until in the extreme case, when one considers indices with a common component, the correlation is very high.

The occipital curvature being much more highly correlated with the Cerebellar than with the Cerebral Index, it might be expected from Pearson's conclusions that there is more "covering" between one or both components of the Occipital Indices and of the Cerebellar Index than between those of the Occipital Indices and the Cerebral Index. The total occipital arc "covers" absolutely the lambda-inion and inion-opisthion arcs, but the latter comprises the smaller moiety of the total occipital arc (Fig. 1). Thus, Sauter quoted the following mean values for the cerebral and cerebellar arcs:

	MEAN CEREBRAL ARC	MEAN CEREBELLAR ARC
Bush (58♂)	66.1	47.6
Bush (43♀)	61.9	46.4
Hottentots (19♂)	64.2	46.8
Griquas (14♂)	66.0	47.1
Swiss (50♂)	62.8	50.8
Swiss (50♀)	58.8	50.2

It might have been deduced from this relation that the upper or Cerebral Index would be more highly correlated with the Occipital Index, since there is more "covering" of the cerebral arc. Instead, this index is much less correlated. An examination of the other component of the Cerebral and Cerebellar Indices suggests the probable answer: the chord in each instance is, of course, also "covered" by the total arc ($S3$), but the lower chord approaches much more closely to the size of its arc than does the upper; this is reflected in the higher Cerebellar than Cerebral Indices. The more closely the cerebellar chord approaches the cerebellar arc, *i. e.*, the nearer the Cerebellar Index approaches to 100%, the more highly correlated would the chord be with the total arc which absolutely "covers" the cerebellar arc. The converse is true in the cerebellar region of the occipital bone. Thus, there is a greater degree of "covering" in the cerebellar region and consequently we should expect a higher correlation coefficient between the Occipital and Cerebellar Indices. Of the biological significance of such a correlation, Pearson and Davin (1924, p. 356) have this to say:

> It seems idle to speak of any such correlations as "organic." There are no, or at best very small "organic" correlations between the shapes of different portions of the skull. The observed correlations are due to the existence of common or covering factors in the indices. And notwithstanding the criticisms which have been raised against the term "spurious correlation," it seems to be the appropriate word to describe these correlations which are due, or largely due, to "covering" factors.

It may be mentioned that the size of the component parts of the occipital bone is decidedly more variable than the total dimensions of the bone. Thus, Morant (1930) showed that the lambda-inion and inion-opisthion arcs are more variable than the total lambda-opisthion arc. In other words, the position of inion is most variable. A corollary of this observation is that two cranial series with the same mean occipital index do not necessarily show the same mean curvature above and below inion. For instance, Early Dynastic Egyptian crania and Slovene crania have a mean Oc. I. of 62.0 and 62.1 respectively; yet, if we subtract the Occipital Cerebellar from the Occipital Cerebral Index, we derive values of —3.4 and +2.1 respectively. Skull series with the same overall curvature may have a stronger curvature above or below inion. Again, we find Serbo-Croatian crania with an Oc. I. of 62.8 and a difference between upper and lower curvatures of +0.8; Greek crania with 62.9 and —1.7; and Telei crania of the Solomon Islands with 63.0 and +3.5. Groups possessing the same occipital index may not possess close affinities in detailed structure.

The same is true even of individual crania within a single racial group. Thus, among my Somali group, crania 2 and 8 both had an Oc. I. of 60.15; yet the Cerebral minus the Cerebellar Index was 90–96.59 (—6.59) and 96.06–95.08 (+0.98). Again crania 22 and 58 had an Oc. I. of 60.31 and differences of the component curves of 96.12–96.33 (—0.21) and 94.78–96.15 (—1.37). Among Bush crania, skulls 1758 and 3731 in the Cambridge Anatomy Department have 59.22 as Oc. I. and 92.9–95.8 (—2.9) and 91.2–98.7 (—7.5) respectively; skull C1369 from an old Hottentot grave at Kakamas, Orange River, has 59.22 as well, but 92.4–95.9 (—3.5). In figures 1, 4 and 5 are shown occipital contours of groups of crania with similar or identical overall occipital indices, but varying from one another in level of inion and other features of their sculpturing.

There is therefore both intra- and inter-racial variability in the relative position of inion and in the relative curvature above and below inion. It is of interest to note that, in the pig, the growth pattern differs between the superior and inferior halves of the occipital bone. Mednick and Washburn (1956, p. 179) comment: "This differential growth potential of the two portions of the occiput . . . is present before birth and is probably genetic in origin."

In short, we see that the occipital indices, while giving an overall picture of the amount of bulging in the occipital region, do not provide any information about the detailed morphology of the occipital bone.

Correlation Between Occipital Curvature and Other Cranial Characters.

There is a rather strong negative correlation between Oc. I. and cranial length (table 2), that is, the longer the skull, the greater the occipital curvature, or vice versa. Clearly this is a "covering" correlation; for part of the very length of a long skull must be due to the occipital prominence, whereas, with a flattish occipital, the skull would seem to have been cut off behind and would be short. Only a single organic process, and not two correlated organic processes, need be postulated. For instance, one genetic process, conditioning a non-expansion or an abrupt posterior termination of the parieto-occipital region of the brain and of its osteogenic and later osseous coverings, would necessarily affect both occipital curvature and cranial length, not to mention a host of other "covering" measurements and indices. One such "covering" measurement is the nasion-opisthion arc, between which and Oc. I. there is a negative correlation of —.2257 and —.2218. Clearly a more bulging occipital (*i. e.*, a lower index) would increase the nasion-opisthion arc.

A strong positive correlation exists between the occipital indices and the cranial (breadth-length) index. A flat occipital (*i.e.*, a high index) makes for a shorter skull, but might leave the cranial breadth unaffected or slightly increased (see below): hence it is not at all surprising to find planoccipital crania tending towards brachycrany.[1] That the growth processes producing occipital curvature are not the only ones involved in the development of brachycrany is clear from the wide difference in *r* between three Australasian cranial series: from .137 in Maoris to .464 in Sandwich Islanders. It must be assumed that other developmental factors are operating to vary the extent to which the cranial index depends on occipital curvature. No amount of occipital flattening would compensate for the lengthening and dolichocephalizing effect of an enormous protruding glabella. Hence, to the extent that other processes are defining the lengthening or shortening, the broadening or narrowing, of a cranium, to that extent will the dependence of, and correlation between, cranial index and occipital curvature be lessened.

An interesting series of correlation coefficients is that between occipital, parietal and frontal curvature. There is a rather strong negative correlation with the parietal curvature. "If the occipital bone is strongly curved, the parietal bone will be relatively flat, and vice versa" (Wagner 1937, p. 72). This is a correlation between two indices which include no common or "covering" components, a type of correlation which, according to Pearson and Davin, should be low. This particular coefficient was not calculated by Pearson and Davin, else they might have recognized it as an exception. They would have found further, though slighter, exceptions in the degree of correlation between the occipital and frontal indices, and still less marked correlations between the frontal and parietal (—.085, —.143, —.145, Wagner, 1937, p. 72). Here, in the parietal and occipital regions are indices without covering components which seemingly break Pearson's rule that "There is no correlation between the shapes of individual parts of the skull."

[1] Since this paper was written, I have received from Professor Mario Cappieri a copy of a paper (Cappieri, 1957), in which he discusses *inter alia* brachycephalization. He stresses that the tendency towards brachycephalization is accompanied by a tendency of the angle between the two parts of the occipital squama, superior and inferior, to become progressively more obtuse, *i.e.*, for the skull to become what we have here called planoccipital. This occipital change is part of a process whereby modern skulls tend towards a rounded form, consequent upon (a) restriction of the base and (b) extension of the calvaria and walls. Cappieri says: "In this process of extension it appears that the parietal bones have the task of conforming to the dynamics of the bones articulated with the base."

Wagner pondered over this correlation between parietal and occipital curvature: he demonstrated that the parietal chord-arc index was negatively correlated with the cranial (breadth-length) index ($r = -.271$ in Australian males, $-.375$ in Sandwich Island males and $+.005$ in Maori males). On the other hand, the occipital index was positively correlated with the cranial index. Since both indices were correlated with head shape, they must be correlated with each other. However, Wagner then goes on to say that "the flattened occipital bone and more curved parietal bone of the Sandwich Islanders and Bainings (of New Britain) are considered to be a *consequence* of their high breadth-length index." (italics mine).

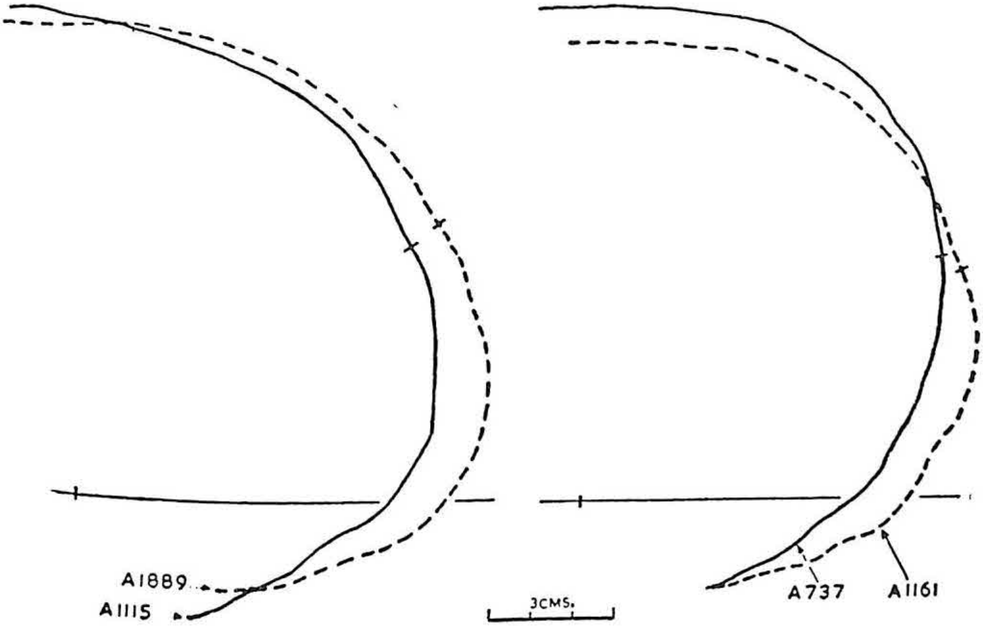

FIG. 2. PARIETO-OCCIPITAL CONTOURS OF PLANOCCIPITAL AND CURVOCCIPITAL CRANIA. NOTE THE INVERSE RELATION BETWEEN PARIETAL AND OCCIPITAL CURVES IN EACH PAIR OF CRANIA.

A 1889:	Xosa cranium with occipital indices	75.0	and	54.4
A 1115:	Bush type cranium with " "	86.3	and	63.9
A 1161:	Bush type cranium with " "	74.9	and	54.4
A 737:	Zulu cranium with " "	93.1	and	84.0.

It is almost redundant to point out that the breadth-length index is not a primarily genetic feature: what is inherited is a growth potential in various directions. It has already been stated above that a single change in the growth pattern of the parieto-occipital region would be enough to account for both planoccipital and brachycranial indices, neither index being the *consequence* of the other. When one considers, in addition, the parietal index, it is not immediately apparent that the *same change* in the parieto-occipital region would bring about high curvature in the parietal region, unless there were an overall genetic or ontogenetic pattern for the entire cranial vault, such that changes in one region of the vault would necessarily be accompanied by compensatory changes in another region.

Evidence is not lacking for such an overall pattern within which regional and compensatory variations may occur. When the occipital bone is larger, the parietal bones are smaller (*r* for bregma-lambda arc (*S*2) and lambda-opisthion arc (*S*3) is —.1587 in males and —.3422 in females—Pearson and Davin, 1924). In other words, although the overall size of parietal and occipital may not vary greatly, the position of lambda between them does (Fig. 3). This observation is in keeping with the more general statement derived by Pearson and Davin (1924, p. 345) from their study of variation in cranial dimensions:

> It seems very noteworthy that the least variable characters of the skull are not the individual bones, but combinations of these which go to make up the broad structure of the skull. There is, we think, little doubt that the variability of measurements on the individual bones of the skull—anatomical as distinguished from anthropometric measurements—must show considerably higher variation than measurements on its structure as a whole. In other words two individuals with closely corresponding anthropometric measurements might have widely different anatomical measurements. The end product might be nearly the same although produced by component cranial bones growing in different proportions.

They go on to deny that this result connotes an important organic law controlling growth; instead they demonstrated algebraically that, from the theory of variation and the definition of the coefficient of variation itself, it can be deduced that "the percentage variation of a character which is the sum of a number of biologically varying units not highly correlated together, will be very considerably less—roughly as the inverse square root of the number of units—than the mean percentage variation of the units themselves. Thus it comes about that the chief anthropometric characters of head and skull have a markedly less variability than the anatomical characters, by which we understand measurements confined to a single bone."

This algebraic proof is perfectly satisfactory when applied to the variation of a *series* of skulls. It does not, however, explain what happens in an individual skull during its growth. The mechanism by which one pattern of occipital growth is associated compensatorily with another pattern of parietal growth is not explicable in algebraic or statistical terms: it must be regarded as a developmental, not a statistical phenomenon, and we cannot escape the conclusion that the correlations do suggest "an important organic law controlling growth." Perhaps Wagner (1937, p. 72) came close to the truth when he suggested in a foot-note:

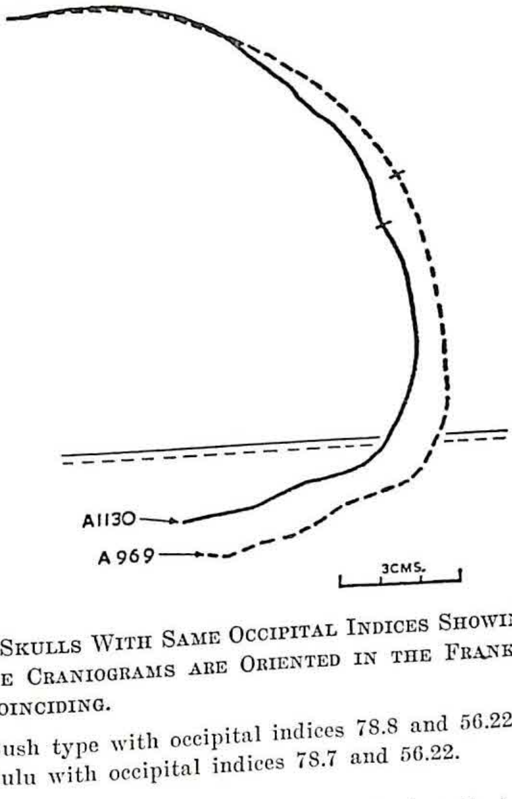

FIG. 3. TWO SKULLS WITH SAME OCCIPITAL INDICES SHOWING VARYING LEVEL OF LAMBDA. THE CRANIOGRAMS ARE ORIENTED IN THE FRANKFURT HORIZONTAL WITH BREGMA COINCIDING.

A 1130: Bush type with occipital indices 78.8 and 56.22.
A 969: Zulu with occipital indices 78.7 and 56.22.

> This negative correlation (between occipital and parietal indices) gives an interesting insight into the etiology of the curvature. Pearson and Davin . . . regard it as "probable that the Occipital Index is a rough measure of the development of the cerebellum" (p. 332). The above correlations, and even more the correlations between the breadth-length and occipital chord-arc indices and Pearson's Occipital Index make it seem likely that it is the cranial shape as a whole

> which is principally responsible for the curvature of the occipital bone. This does not mean that the underlying brain does not play a part in determining the form and size of the cranium. One must reckon with an intimate interdependence of local and general factors acting partly on the brain, partly on the cranium.

His last reference to the brain arises presumably from Pearson's view "that heredity of family or race provides the brain shape, brain size and brain detail, that the function of the cranial bones is to cover this brain, and that their organic correlations are produced in growing to its soft parts. . . . Notwithstanding its plastic character the brain moulds the head shape and size. . . ." Pearson probably oversimplified the position by attributing an exclusive influence to the brain, whereas Wagner, whilst accepting the general influence of the brain, was prepared to countenance direct effects on the cranium, such as those of the temporal and nuchal muscles later demonstrated by Washburn (1947).

A number of "developmental complexes" (Tobias, 1955) must be instrumental in moulding the lineaments of the skull, among which are at least the following: brain-size and form; tooth morphology, size and arrangement; the manner and degree of chewing and the development of the masticatory muscles associated with the nature of the diet; posture, which might influence the neck muscles, the nuchal plane and the occipital bone generally; the degree of development of the para-nasal sinuses; pedomorphic or gerontomorphic tendencies (or the influence of retarding or accelerating mutations and environmental factors); other more specific morphological features probably of primary genetic determination, *e.g.* prominence of brow-ridges, angulation and position of cheek-bones, "overshooting" or "undershooting" of nasal bones (Birdsell, 1951), tori palatinus et mandibularis.

Nevertheless, whatever genetic and other variants may be introduced in the morphogenesis of the skull, the correlations between parts suggest some integrating mechanism affecting the vault as a whole. The manner in which this integrating mechanism operates may become clearer after we have considered the implications for occipital curvature of modern concepts of cranial growth.

Occipital Morphology and Cranial Growth.

Greater or lesser expansion of the supra-occipital, especially the planum occipitale, at the lambdoid suture, must play its part in moulding the curvature of the occipital: for if the upward expansion of the supra-occipital "stops short," then the adequate covering of the parieto-occipital lobes of the brain can be effected only by a downward curve of the parietal bone, especially its hinder part. This is simply a part of the explanation of what our indices have already revealed: the less curved the occipital, the more curved the parietal. If it were assumed that a certain parieto-occipital curvature is necessary to cover the parietal and occipital lobes of the cerebrum, as well as the cerebellum, it would follow that a part of this total curvature is contributed by the occipital bone and a part by the parietal bone. If rather more curvature were contributed by the occipital, rather less would be contributed by the parietal; and vice versa. Now if the planum occipitale of the supraoccipital expanded above only slightly, there would be correspondingly less opportunity for the occipital to contribute to the total parieto-occipital curve. Hence, the "shifting" position of lambda would itself have consequences for the occipital curvature. This line of reasoning presupposes a certain pattern of growth along the lambdoid suture, namely one in which accretive growth may be unequal on the two faces of the suture. Such a pattern is precisely what studies on skull growth have revealed.

The recent change in orientation towards cranial growth and especially towards the role of the sutures has been reviewed by Baer (1954), Moss (1954) and Mednick and Washburn (1956). Moss (1954, p. 334) has summarised the two main schools of thought about the role of the suture as a site of growth:

> One group views the suture as a primary site of growth of the skull, functioning as an "epiphyseal center" (Mair, 1926; Weinmann and Sicher, 1947). Another group regards the suture as only a passive mechanism allowing for relative movement between the bones of the skull during growth (Mijsberg, 1932; Brash, 1934). In this view, the periosteal tissues are the primary growth sites.

Massler and Schour (1951, p. 99) demonstrated by vital staining of the rat skull with alizarin red "S" that growth of the skull vault "occurs primarily by a rapid deposition of bone at the approximating margins of the bones which comprise the various cranial sutures." This was confirmed for the rat by Baer (1954) and for the pig by Mednick and Washburn (1956). Moss (1954) meantime used a variety of extirpative techniques to elucidate further the influence of sutural tissues on the growth and shape of the rat calvaria. He was able to conclude:

> The shape of the calvarial bones is not predetermined by the location of the sutures. It is possible to produce overgrowth of calvarial bones by many methods with consequent displacement of sutural position. This phenomenon appears to

depend on the period of development of the osseous tissue. Calvarial sutures mark the plane of articulation between adjacent bones. The location of the sutures is determined by the relative growth of the adjacent bones.

This is simply another way of saying what Massler (1941—quoted by Baer 1954) had demonstrated much earlier, namely that the amount of growth is not always equal on the two sides of a suture. Massler had shown that the parietals (in the rat) sustain a greater increment of growth than the interparietal at the suture between them.

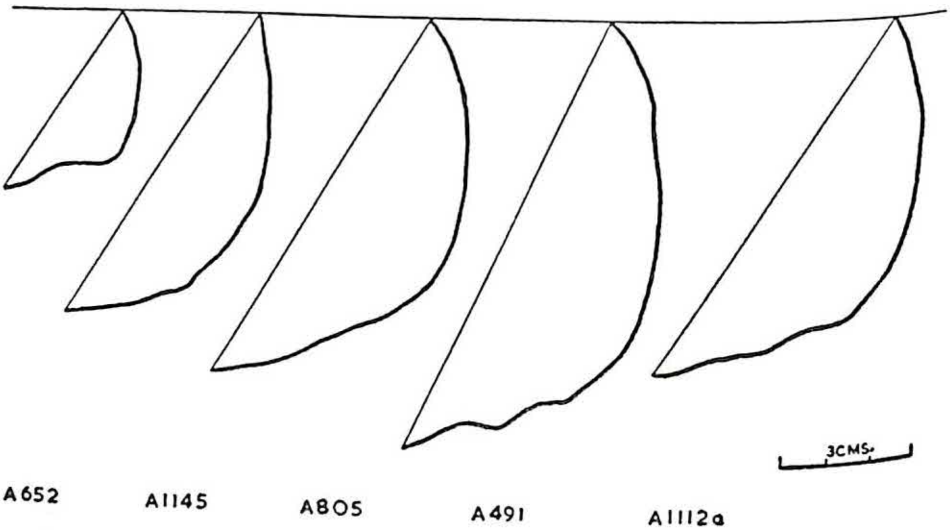

FIG. 4. VARIATIONS OF OCCIPITAL ARCHITECTURE ASSOCIATED WITH SIMILAR OCCIPITAL INDICES. ALL CRANIOGRAMS ARE ORIENTED ON THE FRANKFURT HORIZONTAL.

A 652: Nyasa infant with occipital indices 80.3 and 57.3
A 1145: Immature Bush type with " " 80.0 and 57.05
A 805: Xosa child (ca. 5 years) " " 80.25 and 57.2
A 491: Zulu male with occipital indices 80.1 and 57.2
A 1112a: Bush type male with " " 80.2 and 57.2

On such a conception of skull growth, it is easy to understand the relatively "shifting" position of lambda with the consequences of this shift for occipital size and curvature. Furthermore, Baer (1954) has demonstrated that increase in the height of the occipital bone results from incremental growth at the exoccipital-supraoccipital junctions and at the supraoccipital-interparietal suture. The exoccipital elements begin to unite with the supraoccipital at approximately 45 days, and fusion is completed by 60 days of age, when the occipital complex comprises a single bone. Subsequent increase in height of the occiput results from the slow, continued deposition of bone at the superior margin of the supraoccipital until 100 days of age.

Working on the pig, Mednick and Washburn (1956) showed that in the infant pig (1–8 weeks), sutures are active sites of skull growth, "accounting for the primary extension of the bones of the vault." Later, secondary appositional growth on both ecto- and endo-cranial surfaces, though mostly on the former, is responsible for increasing the thickness of the bones, while secondary resorption occurs between the two tables (not on the endocranial surface), to create the great layer of diploic and sinus spaces in the pig. In a thick-skulled form like the pig, the outer table departs very radically from the architecture of the inner table, so that, while the inner table remains closely moulded to the form of the brain, the ectocranial contour diverges from the brain form most markedly. In thin-skulled forms, such as man and the rat, the outer table closely conforms to the pattern of the inner table, and this in turn to the surface of the brain. Appositional growth in such a thin-skulled form as man is minimal; according to Mednick and Washburn (1956), "this results in the rounded contours of the infant skull persisting throughout the life of man." Therefore, sutural growth is predominant in thin-skulled forms, while apposition and resorption are relatively unimportant in man. It might be inferred from their conclusion that apposition is correspondingly more important in forms of man with heavy bony excrescences like orbital and occipital tori, *e.g.*, palaeolithic man and Australian aborigines: the difference between Rhodesian Man with his heavy occipital torus and a Bush skull with very similar occipital index (A 1161) is at once apparent (Fig. 5). Here, the same occipital index is apparently the result of two different processes: firstly, in Rhodesian Man, heavy appositional growth (probably with a well-curved occipital) and, secondly, in A 1161, marked curvature of the occipital, moulded by sutural growth mechanisms to the underlying brain.

Baer's work has thrown light on another aspect of the problem, the genesis of occipital architecture in general. He has shown that there are certain sites of bone deposition and resorption which are "concerned primarily" with change in the *shape* and *proportions* of the skull, rather than with *dimensional* increase of individual bones. Skull form depends closely on the specific sites, timing and direction of growth. The details of the attainment by the rat of adult occipital conformation need not detain us here (see Baer, 1954, pp. 113-4), but his analysis gives an insight into the morphogenetic processes involved in the curving of the occipital bone. Like the human child's occipital bone, the bones of the immature rat's vault are highly curved: during development a marked flattening of the dorsum of the calvaria occurs (much as the occipital bone of Negroes flattens out with maturation). Weinmann and Sicher (1947) and Brash (1934) believed that the flattening of the arc of each bone is effected by differential apposition and resorption on the outer and

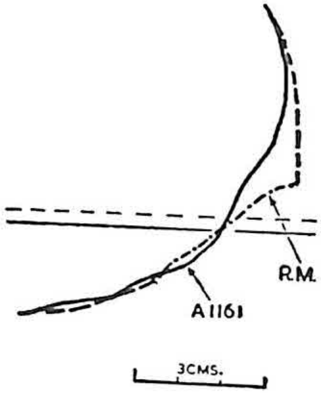

Fig. 5. Extreme Variation of Occipital Contour Associated with Similar Occipital Indices. Both Occipitals are Oriented on the Frankfurt Horizontal with Lambda Coinciding.

R. M.: Rhodesian Man with occipital indices 75.17?? and 54.5??
A 1161: Bush type with occipital indices 74.9 and 54.40.
The portion of R. M. shown by a dot-and-dash line has been reconstructed.

inner surfaces of the vault; but Massler and Schour (1951) and Moss (1954) have pointed out that this endo- and ecto-cranial osteogenesis occurs in the rat calvaria *under 60 days of age*. Sutural growth is simply one part—in a thin cranium, the main part—of periosteal osteogenesis early in development and it enables the bones, which are "floating outwards" (Moss) in the neuro-cranial capsule on the expanding brain, to expand into the presumptive sutural areas. Baer, however, found insufficient evidence of differential apposition and resorption to account for the radical changes in relationship between the cranial bones in the flattening and other alterations of contour, which occur *during the first 100 days of life*. He therefore postulates that the changes in form of the rat skull are the result of *spatial re-orientation of the individual bones*. There are, as it were, hinge-movements between the bones; but hinge-movements of curved bones would make for bumps and disharmony in the general contour of the skull. A harmonizing process occurs. The differential growth of the inner and outer cortical plates observed by Baer at the sutural margins represents the mechanism of adjusting the changing relationship of adjoining bones.

Some such process may well account for the changing pattern of parieto-occipital curvature in man. Ford (1956), working on the fetal human skull, has demonstrated that the parts of the occipital bone do indeed change their spatial orientation towards one another during fetal life. The posterior part of the cranial base undergoes a flattening to compensate for its slower rate of growth, as compared with the anterior part of the base and with the brain and vault. This flattening is achieved by changes in the spatial orientation of the parts of the cranial base and the occipital bone. The angle between the septal and basi-occipital parts of the base opens out from a value of 104.9° at 10 weeks to 122.4° at 40 weeks; likewise the angle between the basiocciput and the foramen magnum (limited posteriorly by the inferior margin of the supraoccipital) increases from 109.0° at 10 weeks to 135.0° at 40 weeks. Finally, the supra-occipital moves to a more horizontal position relative to the cranial base.

The result of these processes is to produce the increasing occipital fullness which is particularly characteristic of man. It may be added, here, that it is these processes which result in the occipital curvature coming into being. For, if the antero-inferior part of the supraoccipital is, as it were, "tucked in" horizontally, the upper part must curve round to meet the parietal bone, in order to close the vault posteriorly. This would account for the marked occipital curvature of infants of different races and it is adequately explained by the type of mechanism postulated by Baer.

We have already seen that the parieto-occipital contour remains relatively unchanged with post-natal growth in the Bushman and related groups. Our measurements have shown that this occipital fullness develops, too, in Negroid infants, but with post-natal growth, is subsequently largely flattened out. If Baer's explanation were to apply to these changes as well, one would expect the occipital bone to undergo a hinging movement downwards about the lambdoid suture; this would be accompanied by differential ecto- and endo-cranial accretion and resorption across the lambdoid suture to smooth the skull contour. Such a downward swing would be feasible only if the underlying brain were meantime undergoing alterations in shape, such that the upper parieto-occipital fullness were becoming reduced, perhaps by a lateralizing tendency. Such a lateralizing tendency would at once account for both the planoccipital contour and the meso- to brachycrany which usually accompanies it. A glance at adult planoccipital crania, however, serves to remind us that the downward hinging of the hinder pole of the skull does not in fact commence at the lambdoid suture. Instead it begins about mid-way between bregma and lambda, so that the posterior part of the parietal bone is itself angulated on the anterior part, to face almost directly backwards (Skull A 737, Fig. 2). This would suggest that the hinging movement at lambda would tend to create so marked a disharmony between parietal and occipital bones as to require, on the parietal face of the lambdoid suture, a large amount of endocranial osteogenesis and ecto-cranial resorption to maintain the smoothness of the general brain-covering.

If there is a lateralizing tendency of the brain, it must then extend over the parietal as well as the occipital lobes of the brain.

Functional Significance of Varied Occipital Morphology.

Pearson always regarded the aim of the Occipital Index as being "to provide some crude measure of cerebellar development" (Pearson and Davin, 1924). In fact, he went so far as to claim that "Among modern man the Negroes stand highest in index and lowest in motor-coördinating capacity." While the latter claim would certainly not stand the scrutiny of modern researches, the whole basis of such reasoning must be questioned. When we are considering different races of man, a big cerebellum does not necessarily imply better co-ordinating capacity. Even if it did, however, it is not at all clear that with a big occipital curve we are *in fact* dealing with a big cerebellum. We are dealing with a total parieto-occipito-cerebral *and* cerebellar conformation: with the relative degree of development and with the shape of the posterior part of the cerebrum and of the cerebellum; with the degree to which the occipital pole overhangs the cerebellum. In a planoccipital skull, the parietal lobe turns sharply downwards just in front of obelion; in a curvoccipital cranium, there is a much slighter deflection from further back in the parietal bone. It does not follow, however, that the parietal lobe housed in a curvoccipital vault is any bigger than that in a planoccipital calvaria. For one thing, the relatively broader vault of a planoccipital skull permits more of the parietal lobes to be housed laterally, as if to compensate for the deficiency of space posteriorly. We can conceive of a re-distribution of parietal lobe material to fit several different surface contours, *e. g.*, those of scaphocephaly and oxycephaly. The change in shape, *per se*, is no more likely to affect the functioning of the parietal lobe than is a variety of sizes within human limits of variation. Likewise, a cerebellum protruding posteriorly is likely to be no more efficient than one well-covered by the occipital lobe.

It is clear that we must abandon once and for all any notion that the occipital index provides a guide to the degree of cerebellar development and, *a fortiori*, to the capacity for motor co-ordination.

We can more confidently assert that the occipital index reflects the degree to which the infantile parieto-occipito-cerebral and cerebellar contour has been retained or departed from, during the post-natal morphogenetic changes in the brain. As to the functional significance of a down-curved parietal lobe and flat occipito-cerebellar contour, in contrast with a slightly curved parietal lobe and well-curved occipito-cerebellar surface, this type of study can throw no light on functional differences of these regions of the brain among various human races.

General Conclusions on Biological Significance of Occipital Curvature.

1. The total occipital curvature is more closely correlated with the Occipital Cerebellar Index than with the Occipital Cerebral Index, presumably because there is more "covering" of the components of the cerebellar index by the components of the occipital index.

2. Within any cranial series, the more planoccipital crania are more likely to tend towards brachycrany.

3. The flatter the occipital, the more well-curved the parietal bone, and vice versa.

4. Both intra- and inter-racial variability exists in the relative position of inion and in the relative curvature above and below inion. Groups of crania and even individual crania of the same series, though possessing the same occipital index, may not possess close affinities in these details of their morphology.

5. The occipital index gives an overall picture of the amount of bulging in the occipital region; it does not provide any information about the detailed structure of the occipital bone.

6. It is suggested that the following growth processes are involved in the establishment of the occipital curvature of the adult human being:

(a) The initial fetal curvature is established by the flattening of the cranial base: as Ford showed, this occurs by hinging movements opening the basi-cranial angles in front of and behind the basi-occiput and between the supra-occipital and the foramen magnum.

(b) Ossification in the brain capsule above the nearly horizontal anterior part of the supra-occipital results in the occipital curving upwards and forwards towards the lambdoid suture.

(c) Accretion of bone occurs on both sides of the lambdoid suture, but the relative amount of ossification on the two sides varies. Hence the position of lambda is variable: the planum occipitale may in consequence be relatively expanded or unexpanded. The curvature of the upper part of the occipital will in turn vary; an expanded planum occipitale is likely to result in more of the general parieto-occipital curve being taken up by the occipital and less by the parietal. If the planum occipitale is small, less of the parieto-occipital curve will be taken up by the occipital and more by the parietal.

(d) Our correlation coefficients have, however, shown that the curvature of the upper part of the occipital (from lambda to inion) is less correlated with the total occipital curve than is the curvature of the lower occipital arc (from inion to opisthion). Some other factor than a shortening and flattening of the lambda-inion arc must therefore be sought to explain the flattening of the occipital arc which occurs in some races.

(e) It is suggested that with post-natal growth, and especially from about the time the permanent teeth begin to erupt, there occurs a re-arrangment of the brain substance of the parietal and occipital lobes. This rearrangement would seem to comprise a relative flattening and lateralizing of these lobes. Such changes are accompanied by spatial re-orientation of the parietal and occipital bones.

(f) The occipital bones hinge downwards at the lambdoid suture; they accomplish this movement without actively compressing the brain. In fact, they probably follow the afore-mentioned brain re-ararngements passively, if the results of all the cited experimental studies on cranial growth apply to man; and there is no reason to doubt that these general principles, though not necessarily the details, are so applicable.

(g) The hinge-movements referred to in (f) tend to create irregularities of contour along the lambdoid suture: these are compensated for by differential ecto- and endo-cranial osteogenesis along the juxtaposed margins of the parietal and occipital bones. The degree to which the parietal bone is involved in the consequent downward angulation is inversely related to the amount of expansion of the supraoccipital along its lambdoid margin; a well-expanded supraoccipital will permit much of the parieto-occipital curve to be taken up by the occipital and only a small amount of the curve will be contributed by the posterior part of the parietal bone. A very flat occipital will result when much of the parieto-occipital downward deflection is in the parietal bone itself. The degree to which the parietal bone turns downward is governed by a proximate and a remote mechanism: more immediately, it depends on the amount of endocranial osteogenesis and ectocranial resorption along the lambdoid margin of the partietal; more remotely, the whole cranial contour must be presumed to be related to the conformation assumed by the underlying brain (Wagner, 1935, 1937).

(h) The hinge-movement, it is suggested, accounts for the very marked reduction in the tempo of cranial length increase which Goldstein (1936) found at this age. At the same time, it is of interest to note that the width increment, both maximum cephalic and minimum frontal, was reduced after five years of age *by a much smaller amount* than were the height and length diameters. It is tempting to attribute this to the lateralizing tendency of the brain and to relate to this phenomenon as well a brachycephalization from about 5 or 6 years of age.

(i) The persistence of the infantile parieto-occipital brain contour into adulthood in some races (*e.g.*, Bushman, Hottentot), and the loss of this contour by parieto-occipital flattening and lateralization, of a *moderate* degree in some human types (*e.g.*, Eastern Bantu and Eastern Hamites) and of a *marked* degree in others (*e.g.*, Western Bantu, Southern Bantu and True Negro), are the main conclusions drawn from a study of the occipital indices of different modern African racial series.

7. It is concluded that the occipital index provides no guide to the degree of cerebellar development and, *a fortiori*, to the capacity for motor co-ordination.

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SOCIAL DISTANCE IN EPIDEMIOLOGY

AGE OF SUSCEPTIBLE SIBLINGS AS THE DETERMINING FACTOR IN HOUSEHOLD INFECTIVITY OF MEASLES

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CERTAIN contagious diseases of man maintain their reservoir of infection solely by a continuous chain of contact between infective and susceptible persons. This is particularly true of infections of the upper respiratory tract that are transmitted through expiration, coughing and sneezing. Many of these viruses and pathogens have short persistence outside the human body and it is, therefore, evident that a successful chain of infection must depend to a large degree on how close the susceptible persons are to the infective ones.

It has long been realized that geographical distance is an important determinant in the spread of contagious disease. The instances of isolated communities that for decades have escaped invasion of common infections, such as measles, and where such invasions when they finally occur take exorbitant epidemic proportions, belong to the classical knowledge in epidemiology. It is also recognized that even in dense urban communities the spread of such diseases may assume a peculiar pattern of movement that confines epidemics of the disease to certain sociologically defined areas in the larger community (*e.g.*, Meyers, 1949). The social contacts of a susceptible person and the kinds of people with whom he is in daily intimate contact are epidemiologically more decisive than the population density of the area in which he lives.

The concept of social distance, although recognized as an epidemiologic principle, has not been used in the exact terminology adapted by sociologists who by meticulous interviewing determine the number of a person's social contacts. Yet, how much simpler would it be to use the attack rates of contagious disease as a unique measure of social contacts between groups than to use a number of graded questionnaire items.

The reverse assumption, however, has long been a cornerstone of the study of the infectivity of diseases. Chapin (1925) established the simple device of *secondary household attack rates* as a measurement of maximum infectivity of the contagious disease. The attack rate is computed as: (total number of cases in household minus primary cases) divided by (total number of susceptibles in the household minus primary cases). The underlying assumption is that susceptible persons living in the same household are at maximum human contact—or at a minimum social distance—and, furthermore, the degree of contact is assumed to be the same in all households in a given epidemiologic area. This measurement has been highly informative in practical appreciations of the difference in infectivity of various contagious diseases. In particular, it has been a useful measurement in field trials of biologic products such as the passive immunization with gamma globulin against infectious hepatitis (Ashley, 1954) and active immunization against whooping cough (Med. Res. Council, 1956). Because of the high expected attack rates at househould exposure, fewer observations are needed to obtain significant rate difference than would be necessary in total community attack rates.

However, there are several limitations in the use of secondary attack rates either as a measure of the maximum infectivity of disease or the social structure of households. In order to arrive at the attack rate, one must be able to determine the susceptibility of the individuals *a priori*. In certain instances, this can be done fairly accurately through the medical history, provided one deals with infections that practically always result in frank disease. Measles and whooping cough are some of the few examples. Other infections that can confer immunity without disease leave the troublesome procedure of measuring antibodies before the household is infected, which limits the study to special circumstances. Retrospectively, in the case of measles, susceptibility can be determined fairly satisfactorily. The only exception concerns infants that, for a certain length of their first year, have passive protection from transplacental antibodies.

Another limitation occurs when several susceptibles in one household are exposed to one or more primary infectives with short periods of infections. The possibilities exist that the subsequent cases among these susceptibles can be caused either by the primary case or occur as a chain where the secondary cases give rise to teritary cases among susceptibles that escaped infection from the primary case but were infected by the secondary case. Such chains will result in a higher secondary attack rate in large households than in smaller households, and the secondary attack rate defined by Chapin therefore becomes a function of the social structure of the community, with higher secondary attack rates in communities with larger family units.

Efforts to correct the secondary attack rate for family size were initiated by Greenwood (1931) who formulated his chain theory as one of the first attempts to introduce the stochastic process in epidemiology. Greenwood's theory is most easily understood in the case of a family with three susceptibles where one becomes a primary case that is infected from the outside. Four kinds of happenings are now possible in respect to infection or escape of the two remaining susceptibles if "p" is the "true" secondary attack rate, and $p = 1 - q$

(0.0)	Two escapes	Probability	q^2
(1.0)	One secondary case	"	$2pq^2$
(2.0)	Two secondary cases	"	p^2
(1.1)	One secondary, one tertiary	"	$2p^2q$

The probabilities refer to the expected frequency with which families with these happenings would occur. Obviously, the sum of the probabilities is unity. The secondary attack rate as defined by Chapin would be

$$\tfrac{1}{2}(2pq^2 + 2p^2 + 4p^2q) = p(1 + pq).$$

Since the term $(1 + pq)$ is larger than unit, the secondary attack rate in families with 3 susceptibles would be larger than that in families of 2 susceptibles, where one would expect a rate of p.

Greenwood was able to confirm this theory on actual observations of Stocks and Karn during measles epidemics in St. Pancras, London (1926). However, the data were collected with the limitation that families were listed by number of cases subseqent to the primary case only, so that the categories (2.0) and (1.1) were indistinguishable. Thus, the observed number of families with no, one, and two subsequent cases were respectively 120, 93 and 86. Solving for p in $p + p^2 - p^3 = (93 + 172)/598 = 44.3$ and computing the expected numbers, he obtained *122.4*, *88.2*, and *88.4*, which is a satisfactory agreement.

However, the Greenwood chain theory received a severe blow, when later Wilson *et al.* (1939) published observations on measles collected from 1928 to 1932 in Chapin's former office site, Providence, Rhode Island. This imposing monograph contains among other valuable data a chapter ("Greenwood's brat and other matters") where the chain theory is tested against data where the categories (2.0) and (1.1) were separated. The distinction was made by careful recording of the time interval between cases. Those families where the two secondary cases occurred with more than 8 days' interval (the incubation period of measles) between onset of typical symptoms were listed as (1.1)—one secondary and one tertiary case—while an interval of less than 8 days placed the family in category (2.0)—two secondary cases.

Actual observations were:

FREQUENCY OF FAMILIES

	Observed	Expected *
(0.0)	51	51.2
(1.0)	67	66.5
(2.0)	262	175.3
(1.1)	36	123.0
	416.	416.0

* based on $p = 0.649$.

It is readily seen that the expectancy in category (1.1) exceeds the observation far more than random errors would permit. Several explanations were offered, of which one published in one of Greenwood's last papers (1949) seems likely. He abandons the idea of a uniform milieu in all families of a community and proposes the infectivity (p) of measles is distributed in a certain way, depending on the family characteristics.

Assume, for example, the simple situation that the community consists of two equally sized groups of families, in one of which the infectivity is 0.50 and in the other the infectivity is 1.0. The frequency of the four categories of families—(0.0), (0.1), (2.0), and (1.1)— would then be 0.25, 0.25, 0.25, and 0.25 in the first group, and 0.0, 0.0, 1.0, and 0.0 in the second group. The total community would then present a frequency of 0.125, 0.125, 0.625, and 0.125. An observer who is unaware of the difference in families, and hence computes the overall infectivity from the latter distribution, finds $p = 0.6621$ and computes the expected distribution in the respective categories as 0.114, 0.151, 0.439, and 0.296, which in most cases would be acceptable with the first two categories but quite unacceptable for the last two categories—the families with two subseqent cases.

The characteristic deviation of that observed data from the chain theory can also be expressed in the fact that the risk of secondary cases always appears higher than the risk of tertiary cases. This phenomena is also found in the case of 4-child families with one primary case. The quaternary attack rates that are occasionally found in these families tend to be higher than the tertiary rate, but with doubtful significance (Abbey, 1952).

Bailey (1953) and later in his monograph (1957) analyzes in detail the idea, suggested by Greenwood, of a variation in the infectivity rate among the household. He found that by assuming a simple distribution of p among the households (Beta-distribution) the data observed in Providence could be fitted for both 3-child and 4-child families with one primary case. The distribution of infectivity was statistically the same in both sets, J-shaped with infinite ordinate at $p = 1$.

Thus, the observed data are compatible with the assumption that circumstances in various households differ to the extent that the risk of a susceptible child contracting measles from its infective sibling depends on environmental factors of the household. It is inviting to speculate on the nature of these factors: size of dwelling, cleanliness, cultural habits, etc., none of which was specified in the observed data.

However, there are data in the Wilson report which will be treated in the following and which can be shown to explain satisfactorily and simply the nature of difference in household infectivity. It rests solely with the age composition of the children, and the clue is contained in Wilson's careful tabulations of the events in the 2-child families.

A re-analysis of the infectivity in 2-child families with one primary case.

Tables 38a, b, c, d, and e in Wilson's reports present 1340 2-child families with one primary case. In 500 of these families the second child was reported to have had measles previous to the primary case. Among the remaining 840 families were 70 families in which the exposed child was less than one year of age. Twenty five of these 70 children contracted measles subsequent to the primary case, giving an attack rate of 0.36 which is considerably lower than the rate in the 770 families with susceptible, exposed children over one year old; 621 of these 770 children became infected ($p = 0.807$). Since it is entirely possible that an unknown proportion of the 70 infants was protected by maternal antibodies, it is necessary to exclude this group from the following analysis of age factors responsible for infection risk, excluding active and passive immunity.

Wilson's tables 38c, d, and e presented the families by age of primary case and age of secondary case (c) of susceptible escape (d), and of immune sibling (e). The data of the former two tables can be arranged in a single table containing the attack rate among susceptible siblings by age of the primary case and by age of the exposed susceptible. While the overall attack rate is $621/770 = 0.807$, inspection of the table shows that there is an obvious dependence of the attack rate both with the age of the susceptible and with the age of the primary case.

Preliminary polynomial analysis.

In order to obtain an estimate of the nature of the dependence of attack rate on age, a polynomial regression analysis was performed with attack rates as the dependent variable on components of age of the primary case (x) and age of the susceptible exposed (z).

TABLE 1

Matrix of normalized sums of products for the computation of co-fficients in Equation (1)

	x	x^2	z	z^2	xz	y
x	4,876					39.24
x^2	71,672	1,207,038				—157.48
z	2,395	42,630	8,547			—177.38
z^2	35,783	684,833	125,194	2,067,504		—3,468.61
xz	50,817	885,698	79,762	1,236,195	1,057,824	—1,153.12
y						43.0778 (135)
					Variance of components	11.2921 (5)

Coefficients	standard error
$b_x + .06078$	.0206
$b_{xx} - .00498$	.0017
$b_z + .00155$	.0185
$b_{zz} - .00398$	.0011
$b_{xz} + .00470$	.0019
b_0 0.6212	.0196

The coefficients in the following equation were determined:

$$y = b_0 + b_x x + b_{xx} x^2 + b_z z + b_{zz} z^2 + b_{xz} xz \tag{1}$$

where y is the expected attack rate.

The matrix of sums of products and sums of squares is given in table 1, together with the coefficients of the variables and their standard error, derived from solving the five equations.

$$S_{iy} = \Sigma\, b_{ij} \cdot S_{ij}$$

where $S_{ij} = \Sigma i \cdot j - (\Sigma i \cdot \Sigma j)/770$; i and j stand for x, x^2, z, z^2, and xz, and S_{ij} are given in the matrix.

The order of magnitude of the coefficients and their confidence limits permit a simplified prediction, as given in equation (2)

$$y = 0.8958 - .00105(x + z - 15)^2 - .00306(x - z - 4)^2 \qquad (2)$$

This equation has almost the same information as that of equation (1). The variance components due to regressions are 11.1819 and 11.2921, respectively. Equation (2) has two essential variables $x + z$, the sum of ages of the two siblings, and the difference in their ages (primary minus the secondary). The latter term provides 85% of the information but the 15% due to the sum of ages is statistically significant.

In table 2 the data from Wilson's tables are rearranged by these two variables, and the fraction of secondary cases over primary cases is given for each combination. The prediction equation (2) consists of two parabolic planes. However, when the sums and difference are large the predicted attack rate attains negative values.

A more natural, but biometrically less manageable, expression is the following:

$$y = 0.8979e^{-0.001(x+z-15)^2-0.005(x-z-4)^2} \qquad (3)$$

which was obtained by iterative procedures. This equation never gives negative values of y. On the graphs of figures 1 and 2 the observed frequencies are plotted by sum of ages and difference of ages, respectively. The lines are drawn with corrections, according to equation (3) for $(x - z)$ in figure 1 and $(x + z)$ in figure 2.

The marginal totals of table 2 are used to plot the points in the graphs.

While the agreements seem acceptable there is need for a test of goodness of fit. Most of the individual fractions in table 2 represent too small numbers for the direct application of the Chi-square test. For such purpose the table was condensed to give reasonably high numbers as in table 3.

The expected numbers of secondary cases were computed by adding the expectancies within each of the 15 groups according to equation (2) and to equation (3). The Chi-square test was now performed for each of the two sets of summed expectancies with the following results:

Equation (2) $\chi^2 = 6.726$
Equation (3) $\chi^2 = 6.722$ with 10 degrees of freedom

TABLE 2

Secondary cases over secondary susceptibles by age of the two siblings
age of primary case—age of susceptible sibling
$(x - z)$

Sum of Ages $x + z$	—18	—17	—14	—11	—8	—7	—6	—5	—4	—3	—2	—1	0	1	2	3	4	5	6	7	8	9	10	13	Total
1												0/1													0/1
2											3/4														3/4
3												2/4		5/6											7/10
4									1/1		0/1		1/1		8/9										10/12
5										2/5		2/2		3/4		10/13									17/24
6									0/2		3/3				15/15		6/9								24/29
7								1/1		5/7		2/4		1/4		18/21		17/19							44/56
8							0/2		1/1		6/6				24/27		20/25		11/15						62/76
9								2/2		3/4		4/5		10/10		26/32		19/21		4/5					68/79
10							0/1		2/2		3/6				35/42		19/24		7/7		3/4				69/86
11						0/1		0/1		3/3		5/5		18/20		33/41		11/12		2/4		1/1			73/88
12									2/2		7/7				21/24		17/19		2/2		1/2		1/1		51/57
13				0/1				1/1		6/6		2/3		7/8		22/22		4/4		1/2					43/47
14							0/1		1/2		5/9		5/5		15/16		11/11		1/2						38/46
15								2/2		4/5		2/5		8/8		8/8		2/3		2/3					28/34
16							1/2		2/2		4/5				8/8		3/5		2/2				2/2		22/26
17								2/4		4/7		1/2		2/2		5/5		1/1							15/21
18					0/1		0/1		4/4		4/6		0/1		5/5										13/18
19						0/2				1/1		5/5		2/2		3/3									11/13
20							1/1		2/3		3/5														6/9
21		0/1						0/1		1/2		1/1		1/1											3/6
22	0/1		0/1										1/1								1/1				2/4
23										1/1				1/1										0/1	2/3
24									1/1		2/3														3/4
25						0/1				1/2															1/3
26					0/1				0/1																0/2
27										1/1		0/1													1/2
28					0/1																				0/1
29						0/1														0/1					0/2
30											1/1				1/1										2/2
33								1/1						1/1				1/1							3/3
35														0/1											0/1
36															0/1										0/1
Totals	0/1	0/1	0/1	0/1	0/3	0/5	2/8	9/13	16/21	32/44	41/56	26/38	7/8	59/68	132/148	125/145	76/93	55/61	23/28	9/15	5/7	1/1	3/3	0/1	621/770

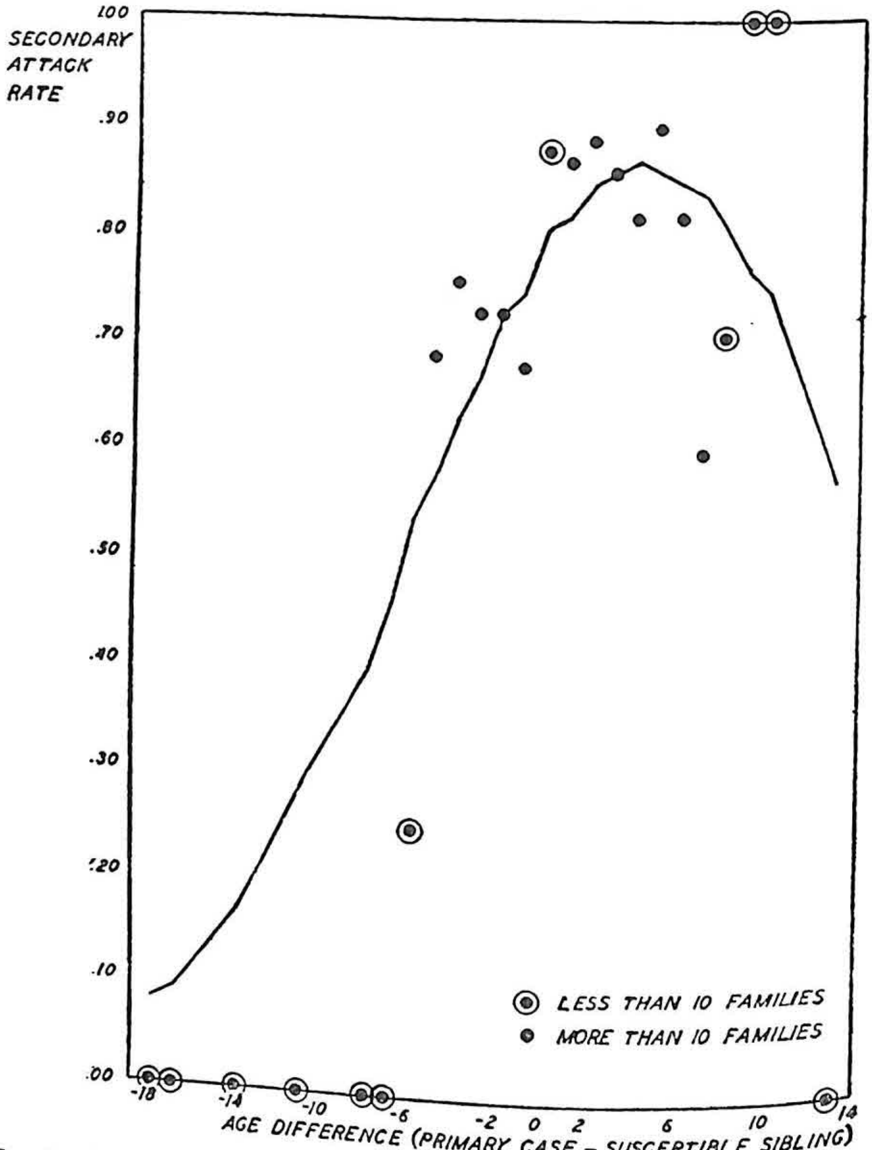

FIG. 1. ATTACK RATES AMONG SUSCEPTIBLE SIBLINGS IN 2-CHILD FAMILIES WITH ONE PRIMARY CASE, BY AGE DIFFERENCE BETWEEN THE TWO CHILDREN. THE CURVE IS DRAWN ACCORDING TO EXPECTANCY FROM EQUATION (3) ADJUSTED FOR THE SUM OF AGES FOR EACH AGE DIFFERENCE.

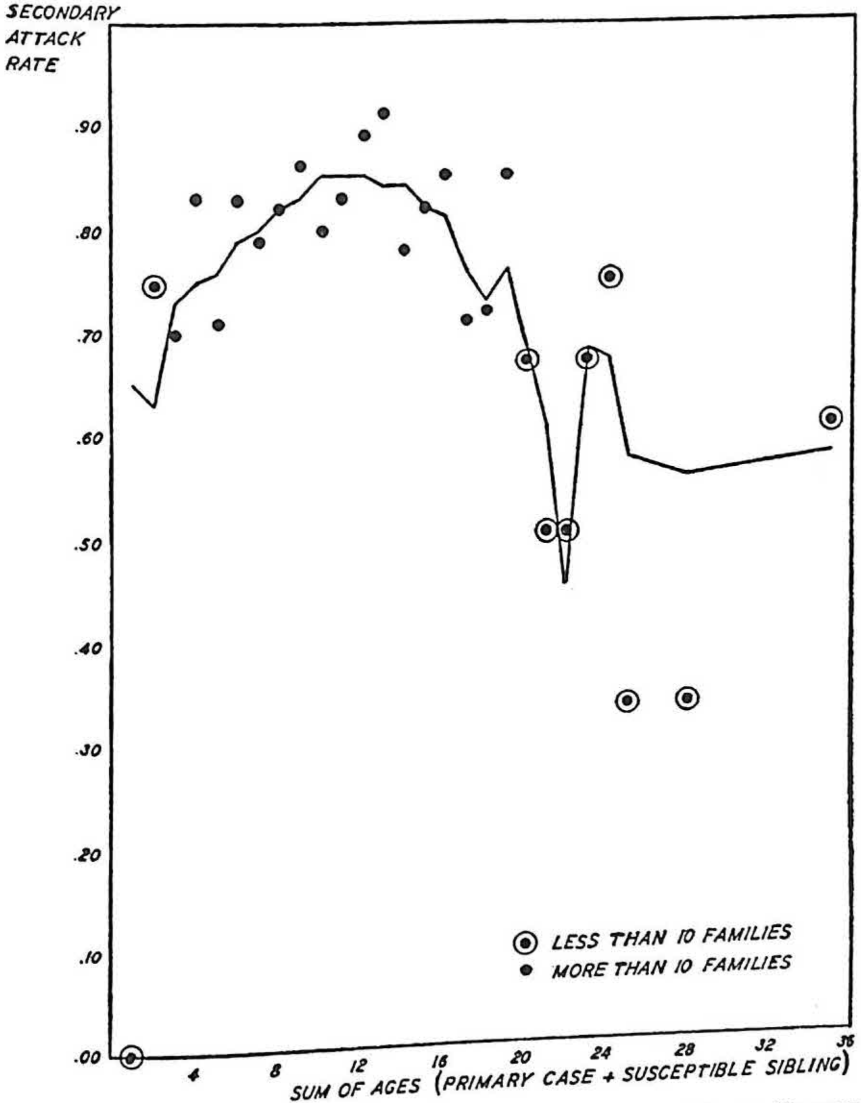

FIG. 2. ATTACK RATES AMONG SUSCEPTIBLE SIBLINGS IN 2-CHILD FAMILIES WITH ONE PRIMARY CASE, BY SUM OF AGES OF THE TWO CHILDREN. THE CURVE IS DRAWN ACCORDING TO EXPECTANCY FROM EQUATION (3) ADJUSTED FOR AGE DIFFERENCE FOR EACH SUM OF AGES.

The minimum Chi-squares obtainable, if the coefficient of $(x+z-15)^2$ is assumed equal to zero, are:

	Minimum Chi-square	Loss of information disregarding $(x+z-15)^2$	
Equation (2)	15.278	8.552	with 2 d.f.
Equation (3)	14.836	8.114	

Because of the significant increase in Chi-square ($p=0.02$) caused by omission of the sum of ages, this component is assumed to be of real influence on the attack rates although less important than the age difference. The Chi-square value of table 3 omitting both components assumes the highly significant value of 48.11 (14 d.f).

TABLE 3

Aggregated attack rate from table 2

	$(x-z-4)^2$			
$(x+z-15)^2$	484–25	16–4	1–0	Total
441–100	14/29	19/25	11/14	44/68
81–49	22/34	54/64	61/74	137/172
36–25	22/34	60/69	64/77	146/180
16–4	49/65	61/72	96/107	206/244
1–0	23/35	41/44	24/27	88/106
Total	130/197	235/274	256/299	621/770

Application of age difference as socio-epidemiological distance to 3 and 4-child families by a simple Monte Carlo method.

The foregoing analysis of infectivity of measles in 2-child households permits the conclusion that the ages of the children are important determinants of the chance of infection. Indeed, in the present material the variation in infectivity among various households is sufficiently explained from the age values in that deviations from the suggested prediction equations are of the order of random variation.

Thus, the variation in household infectivity that was postulated on a theoretical basis by Greenwood, and proven mathematically feasible by Bailey, now seems to lend itself to individual measurement through a simple recording of the age pattern of the susceptible children. Unfortunately, no records have been published of the age distribution of children in 3 and 4-child families in which measles occurred, to the extent of those of 2-child families.

The average ages of the children in the 3-child families are given in Wilson's report but these means seem to include immune children in the families. Table 45 gives the number of families in which the oldest, middle, or youngest child, respectively, was the primary case. The specification is somewhat incomplete but the following information can be extracted, concerning families with three susceptible children:

	PRIMARY CASE		
	Oldest	Middle	Youngest
No. of families	274	110	29
No. with no secondary case	28	16	8
Percent	10.2	14.5	27.5

The chance of escape is highest when the youngest child is the primary case which is in agreement with the terms $(x-z-4)^2$ in equations (2) and (3).

In order to test the actually observed distribution of infection chains in 3 and 4-child families on the basis of age differences between children, it would be necessary to know the exact distribution of the ages in such families and insert the probabilities for each chain which could be derived from the predictions obtained in the 2-child families. This would constitute a rather laborious task, preferably to be computed on a modern electronic machine.

As a substitution for such involved computations, we have tried to approach the problem through a simple card game that has been "played" by students at the Harvard School of Public Health during the last four years* The accumulated outcome of this game is compared to the actual observations on measles in Providence.

Description of card game.

The "players" are divided into groups of three and four, symbolizing families of susceptible children of those sizes. Each player prepares a deck of bridge cards so that he has a distribution of values as follows:

Value	Ace	2	3	4	5	6	7	8	9	10	J	Q	K	
Number	1	1	2	4	4	4	4	3	3	2	1	0	1	Total 30

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* The author gratefully acknowledges the enthusiastic and meticulous participation of the students in Course Epidemiology 7d from 1954-1957.

The value of the cards symbolizes the age of a child and the distribution is roughly that of the age distribution in susceptible 2-child families. The game is played so that there is only one primary case. One of the players is elected to be the primary case. He turns a card at random, the value of which is his age (x). Each other member now turns a card from his deck, showing his age. All those whose ages are within the range $x+m$ to $x-m$ are secondary cases. The distance (m) can be fixed at 3, 4, or 5, the greater the distance the higher the infectivity. It turns out that $m=4$ will give results rather close to the infectivity found in 2-child families.

Tertiary cases will be found among those players who were outside the range $x+m$ to $x-m$ but might be inside a range $z+m$ to $z-m$ where z is the age of any secondary case. In the 4-child families one can continue the chain by searching for quaternary cases among those who had escaped becoming tertiary.

Example:

4-child family $m=4$

	AGE	RESULT
Primary	3	primary case
Sibling A	10	tertiary
Sibling B	7	secondary
Sibling C	1	secondary

B and C are secondary cases because they are within the range of $3-4$ to $3+4$. A is a secondary escape but a tertiary case because he is within the "infective range" of Sibling B.

In this manner the principle of the social distance has been simplified to an all or none phenomenon while the theory assumes a decreasing chance of infection with increased age distance. The asymmetry that was found in the 2-child families symbolized by the term $(x-z-4)^2$ is abandoned and the notion of the sum of the ages has also been discarded. In spite of these rough approximations the distributions obtained by these card games are strikingly similar to those found by actual observation (table 4).

As mentioned previously, the disparity of the measles observations from the Greenwood chain theory is essentially expressed in the fact that the chance of becoming a secondary case (sec. attack rate) is higher than the chance of becoming a tertiary case (tert. attack rate). As seen

TABLE 4

Comparison of family distribution by chain type

Observed (Measles, R. I. 1928-32) and experimental data (Monte Carlo method)

A. Families of three

CHAIN TYPE	OBSERVED (0–22 yrs.)	EXPERIMENTAL $m = 4$	
1, 0, – (a)	49	42	
1, 1, 0 (b)	44	67	
1, 1, 1 (c)	58	60	
1, 2, – (d)	368	251	
Total (N)	519	420	
sec. rate	0.807	0.749	Formula: (b+c+2d)/2N
tert. rate	0.569	0.438	" c/(b+c)

B. Families of four

CHAIN TYPE	OBSERVED 7 mos.–10 yrs.	EXPERIMENTAL $m = 4$	OBSERVED 0–22 yrs.	EXPERIMENTAL $m = 3$
1, 0, –, – (a)	4	18	10	28
1, 1, 0, – (b)	3	24	7	40
1, 1, 1, 0 (c)	1	3	3	7
1, 2, 0, – (d)	8	31	36	71
1, 1, 1, 1 (e)	4	5	4	13
1, 1, 2, – (f)	3	25	3	10
1, 2, 1, – (g)	10	86	13	61
1, 3, –, – (h)	67	204	75	109
Total (N)	100	396	151	339
sec. rate	0.827	0.760	0.750	0.650
tert. rate				
(1 exposure)	0.500	0.509	0.382	0.286
(2 exposures)	0.556	0.735	0.265	0.462
quat. rate	0.800	0.625	0.571	0.650

Formulas:

sec. (b+c+2d+e+f+2g+3h)/3N
tert. 1 (c+e+2f)/2(b+c+e+f)
tert. 2 g/(d+g)
quat. e/(c+e)

in table 3, the 519 3-child families observed in Providence have a computed secondary attack rate of 0.807 with a tertiary attack rate of 0.569. 420 "games" played in groups of three show the same general distribution, although they differ significantly by the chi-square test. However, the "experimental" families show deviations in secondary and tertiary attack rates that are similar to those of the observed measles data.

Similarly, the pattern of secondary, tertiary, and quaternary attack rates in 4-child families is the same in the observed and experimental series, the general tendency being a high secondary attack rate, a lower tertiary, and a higher quatenary. The tertiary attack rate is here separated in the two situations where the escapes can be exposed to either one or two infectives.

Two values of m—"the epidemiological distance"—were tried in the experiments in order to show the influence of an over-all change of the infectivity on the pattern of the three attack rates.

Two hypotheses are then available to explain the discrepancy between observed data and the chain theory: the hypothesis of age as the determining factor, and the hypothesis of the infectivity with undefined family milieu as suggested by Greenwood and Bailey. The essential difference is that the first assumes different infection risks *within the same family*, determined by the number of social contacts that exist between children of various ages. The latter assumes that the same infection risk exists for all members of a given family but that this risk varies between families and, presumably, Greenwood's chain theory would hold within a given family but disagree when several families of a community are aggregated.

By means of the expectancy of risk that was derived from the 2-child family (equation 3) one can compute the probability of events (chain types) in a 3-child or 4-child family with given age distribution.

Example. Given 3 children: A—2 years, B—7 years, and C—12 years. Equation (3) gives the following infection risks:

Probability	of	A	infected	when	B	infective	0.86
"	"	B	"	"	A	"	0.73
"	"	A	"	"	C	"	0.75
"	"	C	"	"	A	"	0.34
"	"	B	"	"	C	"	0.88
"	"	C	"	"	B	"	0.59

Now, assuming that B, the 7 year old, is the primary case, the chain types have the following probabilities:

CHAIN TYPE *	PROBABILITY
1, 0, —	0.06
1, 1, 0	0.25
1, 1, 1	0.18
1, 2, —	0.51
	1.00

The secondary and tertiary attack rates, computed as in table 4, are 0.725 and 0.419 respectively.

Similar computations can be made for a 4-child family with the ages 2, 7, 10, and 12 years. If the 7 year old is the primary case, the probabilities are:

CHAIN TYPE *	PROBABILITY
1, 0, —, —	.017
1, 1, 0, —	.043
1, 1, 1, 0	.021
1, 1, 1, 1	.051
1, 1, 2, —	.055
1, 2, 0, —	.063
1, 2, 1, —	.394
1, 3, —, —	.356
	1.000

with the "attack rates"

secondary	0.717
tertiary (1 exp.)	0.535
tertiary (2 exp.)	0.862
quaternary	0.708

The frequency of the chain types, as computed by this method, changes according to the relative age of the primary case. The overall attack rate is highest when the oldest child is the primary case and lowest when the youngest child is the primary case. In some instances the tertiary attack rate is in fact higher than the secondary attack rate.

DISCUSSION

We have previously used the terms age difference, social distance, and epidemiological distance as almost synonymous, although the relationship between age difference and infection risk was the only demonstrable

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* In the formula of the chain type, 0 means exposed susceptible persons were not infected; dash (—) means either that the susceptibles were all exhausted at this point or that the chain of infection was interrupted because there were no infections in the previous period of infectivity.

fact. The concept of social distance is merely introduced as a hypothesis to explain the mechanism behind the association of relative age and risk of infection. Immunological explanations can hardly be made for this phenomenon since we are reasonably certain to be dealing with persons who are equally susceptible to the disease. It is also hard to believe that age would determine the degree with which the person is infective. For that reason we have resorted to a mechanism that is based upon the social inter-relation of siblings of various ages, and we assume that the direct contact between siblings decreases with age difference even if they are living in the same household. Direct proof is of course needed, but considering the enormous amount of work that is involved in collecting purely epidemiological data, one can easily imagine how complicated a simultaneous epidemiological and sociological study would be. So far the idea of social distance between siblings of various ages is merely based on common human experience.

If we turn to a community of children of approximately the same age in which an epidemic occurs, age difference would probably mean very little to determine the epidemiological distance. In a boarding school of boys between 10 and 14, say, the risk of infection would be related to phenomena of social structure, classes, dormitory, and friendship groups within the school. In this connection it is of interest to point to the work of Abbey who analyzed the sequence of attack rates in several epidemics in such schools. The main result of her work is that a uniform infectivity rate could not be accepted but that it was necessary to assume an oscillation of attack rate during an epidemic similar to the one that can be demonstrated in 3 and 4-child families where the secondary attack rate is higher than the tertiary, etc.

Abbey does discuss the possibility of variation in the contacts among individuals and tries a mathematical model where the population has two sub-populations with less contact between than within these sub-groups. She does find a better fit to the observation with this limited set of conditions but concludes, "However, to obtain conclusive evidence a more realistic model of conditions likely to be found in an actual community is needed. For example, one giving each individual several contact rates." Apparently what is needed in such a study is a simple score for each individual that would be just as workable as the actual age of siblings in small families.

SUMMARY

Reanalysis of data on infectiousness of measles in 2-child families leads to a formula for the probability of infection (y) that includes the age of the primary case (x) and the age of the second, susceptible exposed child (z) as follows:

$$y = 0.8979e^{-0.001(x+z-15)^2-0.005(x-z-4)^2}$$

This expression is in support of a hypothesis that risk of contact infections in households can not be assumed to attain a uniform value, but is dependent on the social inter-relation between children as expressed by their relative age.

This hypothesis explains quantitatively the distribution of infection chains in 3- and 4-child families, that are at odds with the stochastic chain theory assuming a uniform risk of infection. The hypothesis also seems more realistic than the postulate of Greenwood and Bailey which assumes some theoretical distribution of infection risk between families but not within families.

A simple Monte Carlo method, using playing cards, yields distributions similar to those observed in the field.

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THE DEPENDENCE OF PERMANENT TOOTH LOSS UPON TOOTH MORBIDITY

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INTRODUCTION

THE purpose of the present paper is to call attention to a new way of looking at tooth *mortality* (i. e., average number of teeth missing or in need of extraction per examinee). We have adopted the term *lethality* for the tooth mortality per 100 decayed, missing or filled (*DMF*) teeth. Because of marked differences in caries attack rates, standards of treatement and other factors in different populations, we have selected a single example using dental statistics on a single population of Hungarian boys and girls. Comparisons are made only between subgroups of the same population. The chief purpose of this presentation is merely to discuss the usefulness of the concept of tooth lethality in statistical studies of the relationship of loss to dental caries. In the past various statistical aspects of tooth loss have been stuided (Brekhus, 1928, 1929; Knutson and Klein, 1938; Wisan, 1938; Cheyne and Drain, 1940; Munblatt, 1941; Knutson, 1942; Klein, 1943; Hagan and Cook, 1949; among others). In summarizing their findings Finn (1952) states that "loss of the teeth is inversely correlated with the amount of dental treatment received." Mortality of the permanent teeth is therefore recommended as an index of the adequacy and effectiveness of the dental health service rendered to the public. The economic status of the parents was shown to influence the rates of loss of teeth and the ages at which particular teeth are lost. It is also shown that girls have a higher tooth mortality than boys, the difference being a reflection of the higher caries rates among the permanent teeth in girls.

While these statements can be accepted unequivocally, a more thorough discussion of the effects of tooth morbidity upon tooth loss still seems necessary. In our opinion this problem has hitherto not been studied in a fully satisfactory manner since past studies deal only with *tooth mortality*; they correlate loss of teeth only to the number of examinees. In studying the dependence of tooth loss upon the morbidity of teeth it seems a more proper procedure to correlate tooth loss with the number of carious teeth. In this way *tooth lethality* may be computed.

To exemplify this concept we have studied tooth loss data for three urban communities in Hungary (Eger, Gyöngyös and Ózd). Dental caries has been shown to be sex, age and tooth specific (Klein and Palmer, 1941). We have therefore taken these factors into account. The sexes are considered separately. The data are grouped by age; as no differences were demonstrated within age groups if these were grouped by ages 10–11, 12–14 and 16–18, the data are presented in this way. In school age children, only the first permanent molars are lost in sufficient numbers to serve for a statistical analysis. The number of first permanent molars lost is therefore presented for each sex and the various age groups as a ratio of the number of individuals examined and also as a ratio of the number of DMF first molars.

RESULTS

Results for boys and girls in the three age brackets are summarized in table 1. Since the number of DMF first molars varies between 0 and 4, the children in each age group were divided into five subgroups (with 0, 1, 2, 3, or 4 DMF first permanent molars). Tooth mortality and tooth lethality figures are computed for each of the subgroups.

Within the same age bracket, no significant difference is found in the first molar lethality between sexes for children with the same number of DMF first molars. Therefore it seems permissible to unite the data of both sexes to table 2.

In analyzing the data presented in tables 1 and 2, increase of mortality and lethality of the first permanent molar with advancing age is obvious. Since this relation to age is well known from former data in the literature, no further discussion seems necessary.

In comparing the subgroups with increasing numbers of DMF first molars, an increase not only of the mortality but also of the lethality seems obvious although a few data are seemingly contradictory. With increasing numbers of DMF first molars, we find an increase not only in the number of first molars extracted and requiring extraction but also an increase in first molar lethality (number lost per 100 DMF). In examinees with 1 DMF first molar, a lower percentage of this tooth was found to be extracted and needing extraction than in persons with 2, 3, or 4 DMF first molars. A few contradictory findings deserve special attention: in table 1, girls of 16 to 18 years of age with 4 DMF first molars display lower lethality rate than those with 3 DMF first molars, but the difference is not statistically significant. Similarly, an identical lethality figure is evident in table 2, in the 16 to 18 year olds with 3 and 4 DMF first molars. In boys of 10 to 11 years in table 1, and in the total group of this age bracket in table 2, a lower lethality prevails with 2 than 1 DMF first molar; again, the difference is not statistically significant.

TABLE 1

Mortality and lethality of first permanent molars in boys and girls of different age brackets

AGE [1]	SEX	NUMBER OF PERSONS	NUMBER OF DMF FIRST MOLARS PER 100 PERSONS	NUMBER OF FIRST MOLARS EXTRACTED AND REQUIRING EXTRACTION PER 100 PERSONS: FIRST MOLAR MORALITY	LOSS OF FIRST MOLARS PER 100 OF DMF FIRST MOLARS: FIRST MOLAR LETHALITY [2]
10–11	Boys	168	0	0	0
		148	100	5	5.0±1.8
		165	200	6	3.0±0.94
		140	300	16	5.0±1.1
		97	400	28	7.0±1.3
	Girls	111	0	0	0
		117	100	2	2.0±0.92
		179	200	5	2.5±0.83
		156	300	18	6.0±1.1
		140	400	48	12.0±1.4
12–14	Boys	214	0	0	0
		197	100	8	8.0±1.9
		249	200	16	8.0±1.2
		185	300	24	8.0±1.2
		200	400	46	11.5±1.12
	Girls	163	0	0	0
		168	100	3	3.0±1.3
		234	200	17	8.5±1.3
		226	300	28	9.0±1.1
		202	400	59	15.0±1.3
16–18	Boys	333	0	0	0
		160	100	26	26.0±3.5
		134	200	68	34.0±2.9
		118	300	122	40.5±2.6
		120	400	178	44.5±2.3
	Girls	205	0	0	0
		191	100	25	25.0±3.13
		192	200	70	35.0±2.43
		149	300	120	40.0±2.31
		167	400	149	37.0±1.9

[1] Age to the nearest birthday.

[2] Computed to the nearest 0.5 unit.

DISCUSSION

In explaining the phenomenon of an increase in tooth lethality with an increase of tooth morbidity within the same age-group of the same population, several items are to be taken into account.

In Hugary all school children receive treatment through the school dental service without charge. If the school dental service would work optimally, there should not be any loss of first permanent molars in school age children. It is, however, unavoidable that some children escape dental treatment, being absent when examination is carried out by the school dental surgeon or when treatement is to be performed. Furthermore, primary school is entered only after the 6th birthday is passed. Inasmuch as an acceleration of the eruption of the permanent teeth occurs in children in urban communities (Adler, 1957), it may happen that some children are first examined after an unduly long exposure of

TABLE 2

Combined data of both sexes on mortality and lethality of the first permanent molars in different age brackets

AGE [1]	NUMBER OF PERSONS	NUMBER OF DMF FIRST MOLARS PER 100 PERSONS	NUMBER OF FIRST MOLARS EXTRACTED AND REQUIRING EXTRACTION PER 100 PERSONS: FIRST MOLAR MORTALITY	LOSS OF FIRST FIRST MOLARS PER 100 DMF FIRST MOLARS: FIRST MOLAR LETHALITY [2]
10–11	279	0	0	0
	265	100	4	4.0±1.2
	344	200	6	3.0±0.65
	296	300	17	6.0±0.80
	237	400	38	9.5±0.95
12–14	377	0	0	0
	365	100	6	6.0±1.24
	483	200	16	8.0±0.87
	411	300	27	9.0±0.82
	402	400	53	13.0±0.84
16–18	538	0	0	0
	351	100	26	26.0±2.34
	326	200	69	34.5±1.9
	267	300	120	40.0±1.73
	287	400	160	40.0±1.44

[1] Age to the nearest birthday.

[2] Computed to the nearest 0.5 unit.

the first permanent molars, by which time decay is so far advanced that instead of restorative treatment extraction is to be resorted to. In individuals whose first permanent molars have erupted some time before dental examinations and treatment are instituted, the more first permanent molars are carious in a person, the greater the chance that one or another tooth cannot be saved. This would be so even if the progress of dental decay is no more rapid in individuals with more carious first molars than in those with less. Clinical experience points, however, to differences in the progress rate of dental decay, a more rapid progress being displayed in persons with less resistance to caries, i. e. having at equal chronological ages more teeth decayed.

Supposing that absence from school at the time of dental examination and/or treatment is due to chance alone (contrary findings are not known to us), a higher percentage of lethality is expected in persons displaying more than one DMF first molars. Our findings in the age brackets 10 to 11 and 12 to 14 are in best agreement with these expectations.

A surprising finding is recorded, however, in the 16 to 18 year olds. In this age bracket the difference in the lethality between examinees with 1 and 4 DMF first molars is considerably reduced as compared with the same difference in the two younger age brackets. In the younger age groups persons with 4 DMF first molars display a lethality figure that is more than double that of persons with 1 DMF first molar, whereas in the 16 to 18 year olds the increase is only 1.5 fold. On the other hand, the increase in the lethality figures is seemingly more marked from 1 DMF first molar to 2 DMF first molars. We are unable to explain this finding satisfactorily although several possibilities have been taken into account.

First of all, different principles of selection were used in the younger and elder age brackets; in the two younger groups only children born and reared in the same urban area were considered, while in the 16 to 18 year olds all pupils attending the secondary schools were counted. It is well known to us that several pupils were attending secondary schools in the three communities who were enrolled in primary schools in rural districts where no school dental service was available. In this way it might have occurred, inasmuch as rural people lay less stress upon preserving posterior teeth, that several first molars were extracted in this group of secondary school children that would have been saved in urban children of equal age. Since caries prevalence is lower in rural than in urban pupils (Adler, 1954) such extractions were supposed to occur mainly in the subgroups with a lower number of DMF first molars.

However, taking into account tooth decay and tooth loss rates of first molars in children of 16 to 18 years born and reared in the three urban communities—as summarized in table 3—, no difference is conspicuous in comparison with the total population of the same age bracket; rather more uniform lethality figures are seen in the restricted group beginning with 2 DMF first molars. So this explanation was discarded.

TABLE 3

Combined data of both sexes on mortality and lethality of first permanent molars in children of 16 to 18 years, born and continuously resident in the three urban communities

NUMBER OF SUBJECTS	NUMBER OF DMF FIRST MOLARS PER 100 PERSONS	NUMBERS OF FIRST MOLARS EXTRACTED AND REQUIRING EXTRACTION PER 100 PERSONS FIRST MOLAR MORTALITY	LOSS OF FIRST MOLARS PER 100 DMF FIRST MOLARS FIRST MOLAR LETHALITY
142	0	0	0
115	100	22	22±3.86
103	200	77	38±3.38
94	300	102	34±2.77
114	400	148	37±2.33

Therefore, we are more prone to believe that this uniformisation of first molar lethality figures is simply due to a longer exposure. Theoretically, tooth lethality is limited to 100% as only carious teeth are extracted and need extraction in the age brackets examined; in practice, this value cannot be attained. Thus, with longer exposure the tooth lethality curve becomes similar to what is called the saturation curve, this similarity manifesting itself in an equalization of the lethality figures accompanying at first higher, later also lower morbidity figures.

SUMMARY

The influence of tooth morbidity upon the loss rates of the first permanent molars was examined in the 10 to 11, 12 to 14, and 16 to 18 year old school population in three Hungarian urban communities. The number of teeth missing or in need of extraction per 100 decayed, missing or filled teeth (DMF) is called tooth *lethality*. Within the same population, tooth lethality increases with increasing prevalence of dental decay

in the younger age brackets. In the 16 to 18 year olds, however, there is less tendency for tooth loss to vary with the variations in the DMF.

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The author wishes to express thanks to Prof. P. Adler for his aid in this work and in preparing this paper.

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THICKENING OF THE SUBCUTANEOUS TISSUES IN PARALYZED LIMBS IN CHRONIC HEMIPLEGIA

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INTRODUCTION

WHILE measuring the thickness of the subcutaneous tissues of some male patients it was noticed that in some cases of chronic hemiplegia the measurements on the diseased limbs were greater than those on the normal side. In marked cases, the difference between the corresponding areas of the two sides could be readily appreciated by raising a skinfold on the patient's limb between one's thumb and index finger. In order to determine the extent and frequency of such changes, a statistical study of skinfold measurements of 34 male hemiplegic patients in the Infirmary of the Wayne County General Hospital, Michigan, was undertaken.

SUBJECTS AND METHODS

The patients had had hemiplegia for 10 months to 47 years, most of them from 3 to 10 years. Their age varied from 40 to 75 years; the average was 62.9 years. Eleven patients were bed patients. All were survivors of "strokes," with one exception; he had a brain injury 47 years before. During the investigation, tendon reflexes, wrist and ankle clonus, and the Babinski reflex were tested. A central motor disorder (spastic paralysis) was confirmed in each case.

The degree of paralysis of the muscle groups in the limbs was tested and recorded as strong, good, fair, poor, trace and zero (Daniels, Williams and Worthingham, 1956). The word "strong" is here substituted for "normal," because certain groups of muscles in hemiplegic patients "contract strongly against gravity and with full resistance," although movements of the limbs are not "normal." The following muscle groups were tested:

1. Extensors of the elbow.
2. Flexors of the elbow.

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3. Extensors of the wrist and the fingers.
4. Flexors of the wrist and the fingers.
5. Extensors of the knee.
6. Flexors of the knee.
7. Plantar flexors of the ankle and the toes.
8. Dorsiflexors of the ankle and the toes.

Skinfold calipers are commonly used to measure subcutaneous fat, especially in studies of growth, maturation and nutrition. The caliper used in this experiment is a British-made Harpenden skinfold caliper (as recommended by Edwards, *et al.*, 1955). It has a measuring surface of 6 × 9 mm on each jaw; the jaws are approximated by a spring that exerts a constant pressure of 10 gm per sq mm. The skin-fold thickness was measured to the nearest 0.1 mm. The measurement includes a double fold of skin and subcutaneous tissue.

Skin-fold measurements were taken on the normal and diseased sides of each patient over each of the groups of muscles mentioned as having been tested for muscle power. The 8 sites measured were as follows:

1. Back of the arm, half-way between the acromion and the olecranon, with forearm flexed at 90° (over the extensors of the elbow).

2. Front of the arm, same level as 1 (over the flexors of the elbow).

3. Back of the mid-forearm (over the extensors of the wrist and the fingers).

4. Front of the forearm, same level as 3 (over the flexors of the wrist and the fingers).

5. Front of the thigh, 7 inches above the upper border of the patella with the knee flexed at 90° (over the extensors of the knee).

6. Back of the thigh, same level as 5 (over the flexors of the knee).

7. Back of the leg, 3 inches below the skin crease opposite the knee (over the plantar flexors of the ankle and the toes).

8. Front of the leg, just lateral to the shin, same level as 7 (over the dorsiflexors of the ankle and the toes).

FINDINGS

The skinfolds were frequently increased in thickness, sometimes considerably, on the diseased limb of the chronic hemiplegic patients. The absolute and relative differences in skinfold thickness of the two sides were calculated. The averages of these measurements are given in table 1. The relative thickening of the skinfold on the paralyzed limbs vary from 21.5 to 45.0%; it was most marked over the upper arm and least over the forearm and leg.

TABLE 1

Average thicknesses of skinfolds in different regions of the limbs of hemiplegic patients.

	AVERAGE THICKNESS OF SKINFOLD DISEASED SIDE MM	AVERAGE THICKNESS OF SKINFOLD NORMAL SIDE MM	AVERAGE INCREASE IN SKINFOLD ON THE DISEASED SIDE MM	AVERAGE RELATIVE INCREASE ON THE DISEASED SIDE[1] %
Arm, extensor surface	16.0	11.6	4.4	42.8
Arm, flexor surface	7.1	5.0	2.1	45.0
Forearm, extensor surface	6.4	4.7	1.7	35.4
Forearm, flexor surface	8.3	6.7	1.6	28.4
Thigh, extensor surface	19.5	15.2	4.3	37.2
Thigh, flexor surface	13.7	10.7	3.0	33.3
Leg, plantar flexor surface	11.7	9.9	1.8	21.5
Leg, dorsiflexor surface	9.4	7.3	2.1	33.6

[1] The average relative increase in skinfold is computed from the individual relative differences.

The degree of paralysis of different muscle groups is quite variable in any given patient. The upper and lower limbs of the affected side of the same patient may be quite different in the degree of muscular function retained. The extensor and the flexor groups of muscle of the same diseased limb tend to be more related to one another, but it is not uncommon to find that they also are paralyzed to quite different degrees. Owing to this fact, each skinfold and its corresponding muscular group are considered as one unit in the analysis of the degree of thickening of the skinfold in relation to the degree of muscular paresis. In the 34 patients, there are altogether 251 changes in skin-fold thicknesses matched with their corresponding muscular tests.[1] For these cases the degree of difference in skinfold thicknesses in the 6 different degrees of muscle function are shown in figure 1.

[1] A few measurements were difficult to take and the readings are not included. There were also three cases with lower limbs amputated; two cases had amputation before and one case after the onset of the cerebral vascular accident.

In some cases, the skinfold of the diseased side was actually slightly thinner. This reversed difference never exceeded 35%. Nevertheless, the existence of the biological variation which is reflected in this finding should make one wary of interpreting small differences as changes, at least in individual cases.

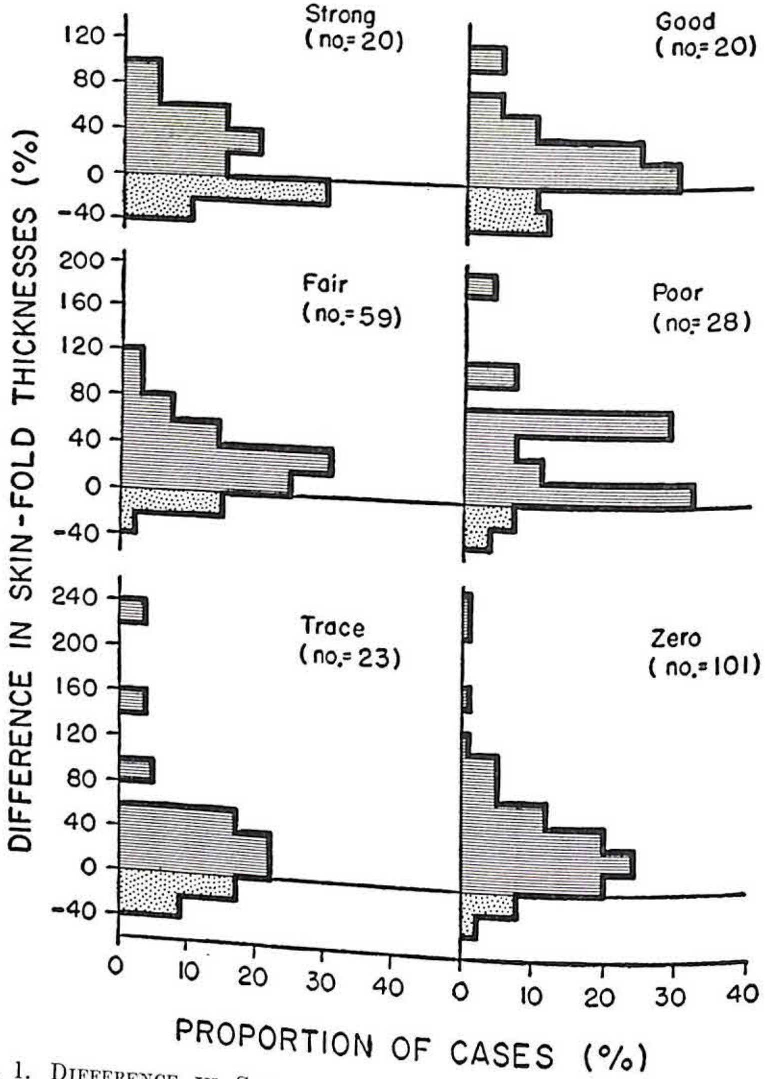

FIG. 1. DIFFERENCE IN SKINFOLD THICKNESS BETWEEN THE AFFECTED AND NON-AFFECTED SIDES ACCORDING TO THE DEGREE OF MUSCLE FUNCTION ON THE AFFECTED SIDE.

On the other hand, the most marked cases of thicknening of skinfolds occured in severely paralyzed limbs. In the 7 cases in which the measurement on the diseased side was over 120% of that on the normal side, the skinfolds all overlay muscles with poor, trace or zero muscle function. Furthermore, there was an average increase in the skinfold of the diseased side of 15.4, 18.9, 28.9, 44.1, 34.0 and 43.3% for strong, good, fair, poor, trace and zero degree of muscle function respectively. This reveals a tendency for more marked increase in skinfold thickening with more marked degree of muscle paralysis. The coefficient of correlation (r) of the degree of paralysis and relative increase in skin-fold is 0.21. This is not high, but the value is statistically significant ($P < 0.01$). To the extent that the evaluation of muscle function is not adequately quantitative and the categories not equal, this estimate of the linear correlation coefficient is conservative.

Duration of the paralysis is another factor that should be considered in respect to the degree of thickening of the skinfold. There are no acute cases to compare with the chronic ones, and the chronic cases can only be selectively compared with one another with exactly the same degree of paralysis. In the present series of chronic cases, there is no detectable correlation between the duration of the disease and degree of increase in skinfold thickness for any of the 6 degrees of muscular paralysis.

DISCUSSION

As was mentioned before, the skinfold thickness represents a combination of a fold of skin proper and the subcutaneous tissue. A thickened skinfold may be due to an increase in one or more elements in it. Skin, for instance, varies greatly in thickness. In a given area of the body, however, individual differences in thickness of the skin proper are small, especially when compared with the variablility of the subcutaneous fat (Richter, 1890, according to Brožek, 1950-51). The actual "feel" during palpation suggests subcutaneous fat increase rather than fibrosis, cellular proliferation or edema. The rate at which a constant tension caliper compresses a skinfold permits the user to some extent to distinguish fat from edema. Furthermore, the amount of tissue fluid could not greatly have affected the differences in the skinfold measurements because pressure from the caliper would have rapidly displaced extracellular fluid. Nevertheless, to substantiate the most plausible interpretation of the present findings, that most of the difference represents a change in subcutaneous fat, post-mortem investigation with gross and histological methods would be desirable.

Even without such confirmation the present study strongly suggests a relationship between the function of a group of muscles and the amount of overlying fat. It is a common notion that physical exercises of a part will avoid or reduce the local deposition of subcutaneous fat, and special devices for women, applying passive exercise to certain regions of the body, are supposed to be effective in local reduction of subcutaneous fat. There has been no direct evidence to support the principle, however, perhaps because of large individual differences such as those in hemiplegic patients.

Brožek (1952) explained that the thicker layer of subcutaneous fat in middle aged than in young adults may be due to decreased physical activities in the middle-aged group. He also measured one group of physically "active" and another group of "inactive" male individuals of the same age, and found that the subcutaneous as well as the total fat content of the body of the inactive individuals is higher than that of the active ones. Although nutritional surplus would be responsible for the high fat content in less active people; yet differential growth of body components also result in relative increase in fatty tissue, to the extent that muscular activities affect muscular development.

Buskirk, Andersen and Brožek (1956) found that in the unilateral active arm of tennis players there is an increase in the length of the long bones of the forearm and hand and stronger muscles on the active side than on the contralateral side, but unfortunately they failed to measure the subcutaneous fat in this study.

Unilateral increase in the subcutaneous fat in some cases of chronic hemiplegia is probably a result of lack of activity accompanying muscular atrophy due to disuse, but it is not specific to this kind of upper neuron paralysis. One victim of poliomyelitis was observed and he also showed considerable increase of subcutaneous fat over the atrophied muscles of the crippled limb.

SUMMARY

In 34 chronic hemiplegic patients the thicknesses of the subcutaneous tissue on the normal and the diseased limbs were measured with a skinfold caliper. Eight skinfold thickness dimensions averaged from 21.5 to 45.0% more on the diseased limbs than on the corresponding region of the normal limbs. The degree of thickening of the skinfold correlates significantly with the degree of muscle paralysis but there is considerable individual variability. The author discusses the suggestion that the unilateral increase in subcutaneous fat in some cases of chronic hemi-

plegia is probably a phenomenon resulting from decreased activity of the diseased limbs.

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FURTHER STUDIES ON THE RELATION BETWEEN HEAD HAIR COLOR AND METALS IN HUMAN HAIR

A correction of the previous paper.

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IN 1956, I published in this Journal an article entitled "Relation between hair color and metals in human hair." This research was helped by two assistants. But at the end of 1956, it was discovered that our results were not reliable and we have been obliged to reexamine those of our previous experiments which were carried out by one of our former assistants. A portion of the necessary corrections was published in "Science."

Reexamination of the metal content of human hair was carried out by dry ashing and paperchromatography, but the results were not as clear as indicated in my previous paper. The wet ashing method and quantitative analyses of metals were also used on human hair. The outcome was also equivocal, *e.g.*, red, Fe 31 (ppm), Cu 49; blonde, Fe 17, Cu 11; brown, Fe 40, Cu 21; black (European), Fe 24, Cu 22; black (Japanese), Fe 59, Cu 29; black (Japanese), Ni 1.5; gray (Japanese), Ni 3.3.

These results indicate that my previous assumption of a correlation between hair color and metal content is incorrect.

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BOOK REVIEWS

Variation of External Body Form in the Individual. By W. W. HOWELLS. 116 pp. Peabody Museum, Cambridge, Mass., 1957. No price indicated. Dittoed.

Professor Howells' monograph is an outstanding contribution to the study of human constitution. Its chief value lies in the highly ingenious methods of analysis rather than in the particular body types which come out of the study.

In the history of somatology, two somewhat divergent streams are apparent in writings both on race and on physique. One stream is largely descriptive and non-metric, and includes both racial typologies and somatotyping. In practice, somatotyping resembles racial classifications in being visual and impressionistic, whilst using standardized photographs instead of visual images. In order to make such descriptive systems more objective, measurements may then be introduced within the accepted framework provided by the types.

The second stream is largely atomistic and quantitative. Racial or constitutional variants are defined on the basis of individually measurable characteristics such as lengths of different parts of the skull and skeleton, and the various indices derived from these. In research on physique, the most elaborate atomistic methods have used devices, such as factor analysis, allowing the student to check "which characteristics go with which" instead of relying upon arbitrary impressions of the salient characteristics of groups of individuals.

Professor Howells' approach suffers neither from the arbitrary limitations of somatotyping nor from the atomism of trait analysis, and he is able to throw light on both. His method is to measure directly the degree of likeness between individuals—or rather, between a collection of measurements taken on the individual—since it is obviously impossible to include all of a person's characteristics. From the direct comparisons between persons (expressed in the form of correlations or in Howells' case, covariances) he obtains factors for persons in the way that trait factor analysis is used for grouping measurements. Howells then studies the measurements in respect to their ability to characterize the groups of individuals which he has already established. The individual comes first, the signs second, unlike the traditional factor method which studies the signs first and only proceeds to the individual (if at all) by the somewhat laborious method of counting their weighted or unweighted scores in the different indicators. Howells finds that people are alike and then discovers the similarity to be in their bulkiness, tallness, shortness and so on.

Previously, Burt in England has used this double approach in studying physique, and examined the special conditions necessary for the results to be identical from which ever end they are applied: i. e., from persons or traits.

In any case, one must arbitrarily choose both the individuals and traits to be measured, and neither form of analysis can give results which are independent of these selections. Howells sampled 30 individuals to be as highly representative of white Army separatees as possible, and chose 50 measurements with the aim of characterizing body form with as little overlap as possible of lengths, diameters and circumferences. Weight, skinfold thicknesses and the dimensions of bones and other internal structures were not included.

All the men had previously been body-typed by the late Professor Hooton, and representativeness here refers to the distribution of these body types. This term does not apply so satisfactorily to the list of body measurements, which are somewhat biased by a preponderance of horizontal over vertical dimensions.

Covariances between each person and all the remainder were calculated by first expressing each person's measurements as deviations from the mean for that measurement, and then summing the deviation products obtained for any pair of individuals over the whole 50 measurements to give a single figure for the relation between each pair, so producing a 30 x 30 matrix of persons' relations. The table of covariances is then analysed into principal components, five of which are discussed. The first and most important component, which accounts for 54% of the total variance, corresponds to the major grouping of individuals. Second and subsequent factors correspond to the groupings left after extracting each preceding grouping in turn. These " person " factors are transformed to measurement factors by multiplying the measurements by the normalised person factors as weights. This method makes it possible to arrive at scores whose assessment depends on the dimensions rather than the individuals.

Factor 1 is strongly loaded with major body girths, while lengths and head measurements have very low weighting. When different combinations of measurements are compared for their efficiency in measuring this factor, the best 12 account for 84% of the variation of persons in this factor. Further, the persons' weighted scores based on these 12 measurements correlates 0.99 with the full score obtained by using all 50 measurements—a very notable gain in economy.

Factor 2 defines centralization versus decentralization of mass (paunch and short limbs versus more strung out mass). It therefore does not correspond with the length-girth dichotomy usually found when factorising body measurements. I believe that this disagreement arises because there are too few lengths available to oppose the influence of the girths (despite their high negative loadings), and hence some girths must join their grouping. The six measurements which correlate most highly with the factor consist of trunk and limb lengths contrasted with waist, axillary arm and ankle girths, which is what Burt would have found.

As was true for factor 1, Howells arrived at efficient regression equations for factor 2 which used far smaller aggregates of measurements: in this case some six to twelve.

Factor 3 differentiates individuals with large chest to waist or hip relationship and the converse: i. e., taper from chest to hips with short limbs—especially in their distal segments versus slighter torso and longer limbs. This suggests a modified mesomorph versus leptosome distinction; fat and lean men are found in either group. Factor 3 differentiates chest size chiefly through muscle development: whereas factor 4 does so mainly through lateral skeletal growth.

Howells' emphasis is mainly on body form, and it is possible that had he used body measurements (such as skinfolds), indicative of body composition, or X-ray studies of bone, muscle, fat and viscera he might have slightly altered his interpretation of the factors. Whether this is so or not, Howells' mathematical analysis is quite unequivocal, and enough data are available in the Appendices to permit other forms of factor analysis to be attempted. Although Howells himself is not yet confident of the real nature of the factors emerging from his computations, he seems to have gone a very long way indeed towards making such a clarification possible.

The second part of the study concerns the relations between the measurements and the typing systems of Sheldon and Hooton, and really brings out the flexibility of Howells' method of analysis. The original constellation of men according to their measured similarities is fixed, and Howells' analysis has provided the principal (orthogonal) components, and it is now possible to examine the type ratings of these men on Sheldon's or Hooton's systems in relation to these axes.

The major axis resulting from both typing systems proved to be more closely related to the classic endomorphy than to the bulk factor of Howells: i. e., it partook also of some of Howells' second factor, but the ectomorphs do not fall at opposite ends of this axis. Whilst endomorphy and ectomorphy can be related to two axes, mesomorphy is not amenable to classification as an entity. Hence Sheldon's system is really two dimensional and the differences between Hooton's first component (fat) and Sheldon's endomorphy can be given. Sheldon's mass is concentrated at the equator (waist) level and rapidly falls outwards, whereas Hooton's corresponds to more general fullness. Hooton's first and third components are strongly opposed; Sheldon's ectomorphy and endomorphy opposed less strongly. Hooton's second component and also mesomorphy have to account for a considerable amount of variation, and the answer may be that both systems fail to take account of *general size* or bulk within which the opposing tendencies are present. Like Howells' earlier work, his present findings suggest that the whole system of somatotyping needs a shake up and a reformulation. Perhaps we on the other side of the Atlantic have been less ready to accept the whole basis of somatotypes than those closely connected with the details of the system.

I could not agree more with Professor Howells when he says "what is needed is some systematic approach which allows the body form to speak for itself, and to make its own statements as to the nature and relative importance of its several kinds of variation." Professor Howells' work has done just that most eloquently.

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Morphological Integration. By Everett C. Olson and Robert L. Miller. xv + 317 pp., 81 figs., 111 tables. The University of Chicago Press, Chicago 37, Illinois, 1958. $10.00.

Morphologically and functionally man is the best known of all vertebrates. In addition to the wealth of detailed descriptive anatomy that is available, the physiologists, functional anatomists and orthopedists have provided a tremendous mass of data on the functioning of the human body. The fossil record for the hominoids is still fragmentary, but it is possible to consider in some detail the morphologic transition from primitive primate to man. In spite of this knowledge, there has been no attempt to analyze, from the standpoint of the integrated organism, the changes in the feeding and locomotor mechanisms that occurred during this transition.

Most functional studies deal with a single morphologic unit such as the femur or a single functional complex like the mandibular joint. The practical reasons for studying a complex organism by parts or systems are obvious, yet there is great danger in forgetting that evolution involves the reaction of the total organism with its environment. But the study of the total organism from a functional or even a purely morphologic point of view presents nearly insuperable difficulties. Olson and Miller have approached this problem in a highly original manner, even though the basic thesis is not a new one: the degree of interdependency (correlation) between morphologic components is directly related to the extent of the morphologic integration of these components.

Briefly, the method, which is entirely quantitative, involves taking a large number of predetermined measurements of a morphologic complex such as a skull, the axial skeleton, or the molar series of a mammal. Frequently hundreds and occasionally over a thousand measurements have been made in a single complex with population samples ranging from 10 to 40 (limited because of the labor involved in the computation or, in some cases, by available fossils). Correlation coefficients were computed for all measurements in pairs, followed by special statistical procedures designed for grouping the measures into "Rho-groups," as expressed by the correlations, and "F-groups" which are based on the Rho-groups and have biological, usually functional, significance. An index of morphological integration provides a numerical value for the extent of total integration (mean level of correlation for measured variants). Working with population samples of adult vertebrates (e. g., *Rana* and *Aotus*), with ontogenetic series (*Rana*), and with phyletic series of both vertebrates and invertebrates (*Pentremites* and *Hyopsodus*), the authors have investigated the inter-relationships of selected morphologic complexes (which they regard as more or less separately evolving units within the organism) in relation to growth, function, and, where appropriate, change through time. They have not attempted to explain some unexpected high correlations like the one between total length of pelvis and fibula length in the albino rat. Could this be the result of chance alone, or does it have some obscure developmental or functional meaning? The relationship between function and changes in the evolving units has been considered for several examples. This approach should be particularly fruitful for studies on the evolution of man and other vertebrates with preserved hard parts.

The methodology developed by Olson and Miller for studying morphological integration is tedious and time-consuming, and it requires considerable sophistication in statistical manipulation. The statistics have been criticized (see Simpson, G. G., 1958, Science, vol. 128, p. 138) and some feel that the results are not worth the effort. There is little question, however, that the book will stimulate thought in new directions, and only time can tell what its impact may be on evolutionary biology. As the authors point out, this method of studying morphology makes it necessary "to cast thinking about morphology into the framework of the concept of covariance." This, in effect, is the basis for their interpretation of integration, an interpretation which, they believe, is not antagonistic to other modern approaches to morphological investigation.

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Physical and Behavioral Growth. Edited by PHILIP J. AMBUEL. 104 pp. Ross Laboratories, Columbus 18, Ohio, 1958.

This pamphlet reports the discussions of the 26th Ross Pediatric Research Conference held at the University of California School of Medicine in San Francisco, October 30-31, 1957. Although the 17 papers are quite brief, they introduce the reader to recent important advances along the broad spectrum of physical and behavioral growth and development.

One is impressed with the results steadily flowing from the longitudinal research programs, many of which are at least 30 years old. The power of the longitudinal method for the study of *process* is repeatedly demonstrated in the interrelationships which can be established among important variables measured in the same persons across time. In this context, individuals from a variety of professional disciplines are able to make contributions of considerable interest to one another, as shown in the brief discussions recorded at the end of each paper. Finally, impressive is the fact that much of the data can be translated into action programs; the research, while not applied research, has obvious applications.

Hilda Knobloch points out the significance of environmental patterns for patterns of growth, particularly as the environmental variations relate differentially to disease conditions. Her finding of a much higher correlation between infant developmental schedules and later Stanford-Binet status for children from a disadvantaged group remains unexplained, as does Dorothy Eichorn's finding of a marked sex difference in body temperatures during the early adolescent years. Sontag opens up the fascinating problem of the impact of mothers' emotional disturbance on the behavorial development of the fetus, mediated possibly by physiological stress and hormone imbalance. Sears contributes a valuable paper bringing out the complexity of relationships in the attitudinal area. Sontag, Harris, Jean W. Macfarlane and Jones in separate papers show how complex this interrelationship can be in intellectual, physiological and physical growth, and in physical performance. What the infant and child does—how he behaves—constructs an environing world which in turn modifies further developmental changes.

In a concluding paper on the philosophy of growth, Frank raises the question as to whether we are struggling to comprehend the phenomena of growth in an inappropriate language. It is true that we are still utilizing concepts, terminology and statistical tools borrowed from physics, and especially mechanics. Yet growth seems to exhibit phenomena not readily reducible to mechanical concepts. The model for analysis of growth processes must of necessity be very complex. Yet it is possible that the basic patterns for the conceptual and statistical analysis of growth processes have been available for a long time; it is possible that our problem is chiefly one of handling the many factors involved. With electronic computers and new processes for handling masses of data and multiple variables at hand, the experimentalist need no longer be limited to analytical models which use single or at most three or four factors in multiple relationships. This model may look grossly oversimplified for what appears to an older generation of research workers as an intricately organized complexity. It may well be that a new generation of research workers, trained from the outset in the thinking that must accompany the operations made possible by high speed electronic devices, will be required to expand the concepts and to apply the resulting new ideas.

DALE B. HARRIS

University of Minnesota

Variables Related to Human Breast Cancer. By Elving Anderson, Harold J. Goodman, and Sheldon C. Reed. xiii + 172 pp. University of Minnesota Press, Minneapolis, 1958. $4.00.

The purpose of this study was to determine whether there is a significant excess of breast cancer in relatives of women selected because they have breast cancer (designated as propositae) and whether any familial tendency to cancer (if such exists) is site specific, or whether it is for cancer in general.

The authors selected the propositae from the Tumor Clinic of the University of Minnesota Hospital and chose the brothers and sisters of the propositae's husbands as controls. Factors such as age, marital status, fertility, residence (whether urban, rural non-farm or rural farm), nationality, etc., were considered. The information was secured by questionnaires sent out to relatives of the two groups, by personal interviews, and by use of death records and hospital reports.

The following criteria and procedures were used for the analysis: (1) proportional death rates (the percentage of deaths in individual age groups that are attributable to breast cancer); (2) mortality and morbidity rates in relatives; (3) a direct age-adjusted comparison between cancer and control groups; (4) a comparison of the morbid risks for cancer in the two groups; and (5) sequential analysis, applied to the data on sisters only.

With the exception of the sequential analysis method, all analyses showed a statistically significant excess of breast cancers among *sisters* of breast cancer propositae as compared with the controls. The *mothers* showed an excess of breast cancers only if the cases reported as breast cancer but not verified by official records were used.

To obtain information on *daughters* of breast cancer propositae a second group whose breast cancers were diagnosed about 25 years ago were chosen. This allowed a span of a generation during which their daughters might develop breast cancer. As a control for this group, the wives of the sons of the propositae were chosen. As the breast cancer rate is much higher in single than in married women, the single daughters were excluded from the group, and only married daughters were compared with sons' wives. They showed three times as much breast cancer as the controls.

This study, like all such researches, will prove of interest in its entirety mainly to the limited number of workers in the immediate field, although some of the conclusions will be of general interest: Breast cancer probably includes a group of conditions heterogeneous in etiology, which may ultimately be separable into groups based upon pathological or biochemical differences, in which case genetic analyses will have to be made for each type.

The authors state that the genetic factors depend so much upon environmental factors for expression that it is difficult to demonstrate their effect. To the reviewer, the facts brought up in this volume do not support such a conclusion. If, however, the authors mean that it is difficult to demonstrate the *number* of genetic factors involved or the *mode of inheritance*, the reviewer is in complete agreement.

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The Aging of Populations and its Economic and Social Implications. United Nations (Population Studies No. 26), vii + 168 pp. New York, 1956. $1.75.

In order to avoid any overlapping with functions of the specialized agencies of the United Nations, this report specifically disclaims any intention to deal with employment of older persons or the medical aspects of aging. Thus, it is strictly limited to the field implied by its title.

It is composed of 4 chapters, dealing with "trends and differentials in the aging of populations," "causes of the aging of populations," "economic and social implications," and "future trends," together with an introductory chapter and an appendix in which detailed statistical data are given. Different kinds of aging of populations are distinguished by referring to the "age pyramid" which is conventionally used in census reports and demographic texts to represent the age distribution of a population. Thus, an increase in the proportion of aged persons is described as aging "at the apex" and a decrease in the proportion of young persons is described as "aging at the base."

Contrary to popular belief, the reduction of mortality as it has occurred historically has had little effect on the age structure of populations. It has led to an increase in the proportion of young persons and has left the apex of the pyramid practically unchanged. On the other hand, the reduction of fertility has led to considerable aging both from the base and at the apex. For the time being, thanks to the advances of medical science and to economic and social development, the underdeveloped countries are experiencing a reduction of mortality without any decline in fertility. There is thus a rejuvenation from the base, the apex of the pyramid remaining unchanged. It is reasonable to suppose, however, that mortality will not continue to decline indefinitely without leading to a decline in fertility which will give rise to heavy aging from the base and at the apex. It can, therefore, be forecast that in the underdeveloped countries a phase of rejuvenation will be followed by a phase of heavy aging. A process of this kind is now occurring in Japan.

Chapter 4 is devoted to the study of future developments in the countries which are in the vanguard of demographic aging, that is, those with low fertility. In most of these countries, the decline in fertility, which began over a century ago, seems to have halted. It has been shown that the decline in fertility was responsible for demographic aging and it might be expected that demographic aging would now slow down. However calculations show that the full effects of the decline in fertility in previous decades have not yet been felt in the age structure of the populations and, although fertility has been stabilized, the populations will continue to age for some time. Moreover, although the decline in mortality in the form in which it has occurred has caused little change at the apex of the pyramid, there is reason to believe that this will not be the case in the future. If mortality continues to decline in countries where it is already low—and there is every reason to hope this will be so—it will decline particularly in the upper age groups. The rates in the younger age groups are now so low that it would be unreasonable to hope for appreciable gains at those ages. The low-mortality countries are also those where fertility is low. It follows that in those countries a future decline in mortality will normally result in aging of the population.

Department of
Health, Education and Welfare,
Social Security Administration,
Washington, D. C.

T. N. E. GREVILLE

The Fertility of American Women. By WILSON H. GRABILL, CLYDE V. KISER and PASCAL K. WHELPTON. xvi + 448 pp. Wiley, New York, 1958. $9.50.

Social and Psychological Factors Affecting Fertility. Vol. 5. Edited by P. K. WHELPTON and CLYDE V. KISER. vi + 285 pp. Milbank Memorial Fund, New York, 1958. $1.00.

The first of these books is devoted to an examination of the nature of the factors affecting trends and variations in human fertility in America from Colonial times to the present. Most readers will probably be surprised to learn that from the Colonial and early Federal periods American fertility has been among the world's highest.

The immense amount of data is reduced to manageably useful proportions by analyzing them by century, year, residence, ethnic group, socio-economic status, education, marriage and child spacing, and so on. The authors seem to have missed nothing, and the numerous tables and figures are stuffed full with illuminating facts. If there is a criticism it is that the explanation of terms is not quite comprehensive enough. There is a summary, a fine bibliography, and an index.

I do not think it is too much to say that this is probably the most satisfactory monograph on American fertility to be published in our time.

The second volume represents the concluding reports and summary of the chief findings from the Indianapolis study. This pioneering study to learn something of the social and psychological components of human fertility was initiated in 1938. Twenty-four articles reporting the results and analyses of hypotheses have already been published. In this fifth and final volume of reports Drs. Edgar F. Borgatta and Charles F. Westoff open with a discussion of scaling techniques and factor analysis as applied to the materials for all couples and for planned families. Drs. Ruth Riemer and P. K. Whelpton report on the attitudes of women toward personal freedom in relation to fertility—a report which shows the inherent difficulties in such studies, but nonetheless valuable for all that. Potter and Kantner report on hypotheses relating to childhood situations and conformity to group patterns, and test some reformulated hypotheses on the influence of siblings and friends on fertility. Pratt and Whelpton discuss the hypothesis concerning interest in and liking for children, and extra-familial participation of wives in relation to interest in children, fertility planning and size of planned family. Solomon, Clare and Westoff present data on two hypothecated reasons for the second child, and the data on the more general hypothesis that the interest of children in, and their desire for, brothers and sisters affect the size of the family. Musham and Kiser discuss the data on children desired by wife and husband at the time of marriage in relation to fertility planning and actual number of children 12-15 years after marriage. In the final article the editors summarize the outstanding findings of the Indianapolis Study and discuss some of the methodological lessons for future investigators.

If the Study opened up more questions then it answered, it nevertheless managed to throw a considerable amount of light upon the nature of the problems and the processes involved. It was a study well worth undertaking, and has been brought to a conclusion in a manner which deserves our thanks and congratulations.

Princeton, N. J. ASHLEY MONTAGU

Of Stars and Men. By HARLOW SHAPLEY. vii + 157 pp. Beacon Press, Boston, 1958. $3.50.

"I don't pretend to understand the Universe—its a great deal bigger than I am . . . People ought to be modester." Thus spake Carlyle. Dr. Harlow Shapley has a section in the last chapter of this encouraging book entitled "Humility Comes Naturally" which, however, gives little or no support to Carlyle's dictum. But then, Carlyle was a pessimist, while Shapley is an unreconstructed optimist. In this quite fascinating volume Shapley shows us that in the moment of Time in which the human species has existed it has already come to know an astonishing number of things about the un-Carlylean universe, and that there is every promise—providing we last long enough—that we shall know a great deal more. We may not ever know everything about everything, but there is some chance that we may eventually come to know everything about some things. But that is not the burden of Shapley's message. Shapley wants us to achieve a proper perspective on ourselves. Copernicus turned us from geocentricity to heliocentricity, in our own time we have advanced toward a certain degree of anthropocentricity. Shapley, I suspect, wants to ward off the advance to egocentricity of which so many of us, whose brains so to speak have gone to our heads, show dangerous evidences. Toward this end Shapley shows us how incidental our little world is in the cosmos, that in all the millions and millions of galaxies we are on the very periphery of the outskirts of a quite ordinary galactic system, and that it is highly probable that not only are we not the only form of intelligent life in the universe, but that the universe simply teems (or is it "bristles"?) with vast numbers of intelligences occupying vast numbers of different planets. A conservative estimate would put the number of such biotic planets at not less than one hundred million. There is not the least reason to doubt that some of these intelligences may be a bit brighter than *Homo sap.* They may be endowed with all sorts of senses that we do not even dream of. In short, the probabilities are that in the metagalactic universe we are not alone. I somehow find this a comforting thought. It is something to look forward to: getting to know our neighbors in the metagalaxy.

The probabilities of any extra-terrestrial forces putting an end to this planet are quite as low as those for the terrestrial ones, with the one exception of man himself—man's worst enemy. For this reason, though it may not be generally known, Shapley has always been one of the most active supporters of the social sciences, for as he realizes, it is more important just now to get to know something about the inner constitution of man than it is about the inner constitution of the stars.

Plutarch tells us that when Alexander heard from Anaxarchus that there was an infinite number of worlds, he wept. When friends inquired the cause of his tears, he made answer: "Do you not think it a matter of lamentation that when there is such a vast multitude of them, we have not yet conquered one?" We have yet to conquer ourselves. Can we do it? Of course we can, but the question is, will we? Books such as Shapley's make us feel that it is very much worth trying.

Ashley Montagu

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A Textbook of Psychology. By D. O. HEBB. 276 pp. W. B. Saunders, Philadelphia, 1958. $4.50.

The Behavior of Man, an Introduction to Psychology. K. U. SMITH, and W. M. SMITH. 516 pp. Henry Holt, New York, 1958. $6.95.

It is not ordinarily the policy of HUMAN BIOLOGY to review textbooks. But these are not ordinary textbooks. Your book review editor saw a unique opportunity in requesting a comparison of these two contributions to the growing scientific study of the biology of behavior. Hebb's compact book is an innovation or reversal of trend in the introductory psychology field, and to the casual browser it looks like a study in contrasts when placed beside the Smith and Smith volume. The Smith brothers have achieved a distinctive and distinguished innovation in visual education.

The central point of view of these two books is as nearly identical as independent authors are likely to achieve at this youthful stage of psychological science. They write with a broad scope, yet carefully select within each topical area those experimental and observational data which support theoretical generalizations. The emphasis is on what Hebb calls the method of *behavioristics.* Both books are thoroughly up to date. They do not slight any of the major methods of investigation. Description, quantitative measurement, experimental methods, and correlational techniques are all represented both in sections dealing specifically with the questions of methodology and in a rich coverage of empirical findings. It is difficult for any worker in psychology, and much more so for persons in related biological and social sciences to keep abreast of current research in each of the several major areas of study. Either of these texts can bring the reader an overview of current knowledge which should substantially reduce his feeling of being "out of touch" with the newer developments.

Both books draw freely from the experimental literature on animal behavior, and from physiological psychology regardless of the organism used for the investigation. The development of human behavior is a topic explicitly recognized in chapter headings of each book. Hebb's title for this coverage is: "Heredity, maturation, early learning." He reaches this topic in his sixth chapter, following two chapters on definitions, methods, and interrelations between concepts, and three chapters on neurophysiological mechanisms in behavior. Smith and Smith follow a similar organization, placing their explicit discussion of bodily processes in chapter 4, to precede their treatment of the development of behavior. The close relationships between behavior and its organismic foundations are manifest through most of the remaining 13 chapters of Hebb and 17 chapters of the Smith book.

These two books are fundamentally suited to the same purpose: to tell the scientific story of behavior to those who have only the general liberally educated layman's acquaintance with it. Smith and Smith have found a little more space than Hebb to devote to the topic of frustration and to the practical problems of adjustment. They have also included more descriptions of experiments, and their index includes a somewhat larger number of names of investigators. But even with this they relegate suggested readings, study questions, and projects to a Workbook designed to supplement the text. Original sources for materials discussed and illustrated in the text are listed by chapter. In this respect both books are fully self-contained and most satisfactorily documented.

There are a few major differences of emphasis between the two texts. Hebb has more to say about recent work on the "arousal system." He is brief in his explicit treatment of personality, covering this topic in 11 pages, whereas Smith and Smith gave it four chapters (Part III of their book). They also devote separate chapters to "thinking" and "verbal behavior." Hebb gives a brief discussion of communication and language as a small section within his chapter on "problems relating to thought." Those readers who know Hebb's research and his theoretical writings will find brief expositions of concepts which he developed in detail elsewhere. The "cell assembly" construct and neurophysiological assumptions concerning the diffuse conduction that underlies higher brain processes are explained in sufficient detail early in the book to permit reference to these in later chapters. A continuity is achieved in this way between sensory processes, learning, perception, and thought. The basic assumptions deal with how brain processes are affected by use. Smith and Smith deny that these assumptions can be put to direct test and Hebb is in full agreement. The choice that Smith and Smith make is to stay closer to the classical data of psychophysics, while Hebb bluntly states his position: "Current physiological knowledge is of course incomplete, and some flexibility is needed when we apply it to psychological problems" (p. 103). In his final chapter on scientific method he further defends neurologizing, as he is also quite willing to call it, as a sophisticated statement of ignorance, whose chief function is to permit tentative statements of relations between facts which must ultimately be inter-related, so that this ignorance can be reduced. The facts calling for this relating are, of course, the fact of behavior and the other facts of biology. With characteristic candor Hebb asserts that man shows less need for prefrontal cortex than does monkey. This is obviously a challenge to those engaged in the development of tests for higher brain functions.

A final word is in order about the innovation of Smith and Smith. Their use of visual art achieves a good deal of what they wanted. Illustrative drawings and photographs give meanings to obscure bodily processes, abstract generalizations, and quantitative relations. To the sophisticated student they may often be superfluous, but to the less experienced they will enliven greatly a subject that is complex and fraught with difficulties of vocabulary.

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BOOK NOTICES

USES OF EPIDEMIOLOGY. By J. N. Morris. viii + 135 pp. William & Wilkins Co., Baltimore, 1957. $4.00. As the author indicates in the Preface, a more accurate title of this book would be "Some Uses of Epidemiology in the Study of Non-Communicable Diseases." This makes the volume a good deal more relevant to students of human biology.

FOUNDATIONS OF EMBRYOLOGY By Bradley M. Patten. xvi + 578 pp. McGraw-Hill Book Co., New York, 1958. $10.50. Written with the objective of furnishing a suitable text for a first course in embryology at collegiate level, the volume stresses the similarity of the major developmental processes in the vertebrates. Abounds in informative illustrations.

CLINICAL EPIDEMIOLOGY. By John R. Paul. xii + 291 pp. University of Chicago Press, Chicago, 1958. $5.00. An introduction to epidemiology, defined here as a science concerned with the *circumstances* under which people get sick or remain sick. This includes not only the microbiological and toxicological circumstances, but also genetic and somatic factors (including obesity and body build), and may require the scrutiny of social, religious and political influences.

PERCEPTANALYSIS. By Zygmunt A. Piotrowski. xvii + 505 pp. Macmillan, New York, 1957. $6.75. As the subtitle indicates, this volume presents "a fundamentally reworked, expanded, and systematized Rorschach method," a projective technique used widely for evaluation of personality in the clinical context and extended to field studies.

PRINCIPAL NERVOUS PATHWAYS. By Andrew Theodore Rasmussen. ix + 73 pp. Macmillan, New York, 1952. $5.50. A reprinting of the 4th edition of the well known neurological charts and schemata.

PSYCHOENDOCRINOLOGY. Edited by Max Reiss. vii + 208 pp. Grune & Stratton, New York, 1958. $7.00. Proceedings of a symposium arranged with the aim to appraise recent progress made in the elucidation of endocrine function in psychopathology, both in regard to therapy and to pathogenetic analysis of mental disease.

PARAPSYCHOLOGY: FRONTIER SCIENCE OF THE MIND. By J. B. Rhine and J. G. Pratt. ix + 220 pp. C. C. Thomas, Springfield, Ill., 1957. $4.75. A survey of the field, the methods and the facts of extrasensory perception and psychokinesis research, presented as a one-volume summary of present knowledge about parapsychology.

COMMUNICATION, ORGANIZATION, AND SCIENCE. By Jerome Rothstein. xcvi + 110 pp. Falcon's Wing Press, Indian Hills, Colo., 1958. $3.50. A physicist's excursion into philosophy of science, with information-organization theory as the core and sallies made into several problem areas (scientific theory as a system for prediction, physical models of behavior of organisms, and the possibility of constructing a universal language).

JUVENILE DELINQUENCY. Edited by Joseph S. Roucek. 370 pp. Philosophical Library, New York, 1958. $10.00. A collection of surveys, written by specialists and concerned with the various facets of a complex and significant social problem. The 4 sections analyze the dimensions of the problem and the legal aspects, causes (including considerations of the biological basis of juvenile delinquency, pp. 59-78), solutions and international trends.

Instinctive Behavior: The Development of a Modern Concept. Transl. and edited by Claire H. Schiller. xix + 328 pp. International Universities Press, New York, 1957. $7.50. Selection of papers documenting the approach and accomplishment of leading European students of "ethology," the comparative study of animal behavior, with special emphasis on environmental influences activating and modifying innate action patterns.

ESP and Personality Patterns By Gertrude Raffel Schmeidler and R. A. McConnell. xiii + 136 pp. Yale University Press, New Haven, 1958. $4.00. A serious, methodologically sound contribution to the study of the process referred to as "extrasensory perception." Focussed on personality characteristics in relation to performance in a card-guessing task.

Testing of Negro Intelligence. By Audrey M. Shuey. xv + 351 pp. J. P. Bell Co., Inc., Lynchburg, Virginia, 1958. $4.00. A detailed summary of about 240 studies on Negro-White differences in mental-test performance, carried out in the U. S. and covering a span of over 40 years.

Principles of Genetics. By Edmund W. Sinnott, L. C. Dunn, Theodosius Dobzhansky. xiv + 459 pp. McGraw-Hill Book Co., New York, 1958. $6.75. Revised edition (1st ed., 1925; 4th ed., 1950) of an outstanding perennial senior-graduate textbook. A chapter on Statistical Inference in Genetics was contributed by H. Levene. Human heredity is given due consideration.

Essentials of Human Anatomy. By Russell T. Woodburne. viii + 620 pp. Oxford University Press, New York, 1957. $12.50. Stimulated by the need for a re-examination of the teaching methods in anatomy, resulting from the steadily increasing knowledge in the basic medical and clinical sciences, the author provides a textbook which presents—concisely but without faulty oversimplification—the basic features of the systems of the body, followed by a detailed regional presentation of body structure.

Dynamics of Behavior. By Robert S. Woodworth. x + 403 pp. Henry Holt and Co., New York, 1958. $5.00. Appearing 40 years after the author's *Dynamic Psychology* and supplementing his classical portrait of *Experimental Psychology* (1938, 1954), the volume covers motivation, learning, perception, and problem solving, with emphasis on their interaction.

Measurement of Levels of Health: Report of a Study Group. World Health Organization Technical Report Series No. 137. 29 pp. Columbia University Press, New York, 1957. $.30. Tackling an important but difficult task, the study group classified the *health indicators* as those provided by vital statistics (crude death rate, infant mortality, deaths from communicable diseases, life expectation), related to relevant aspects of physical environment, and concerned with health services (such as availability and use of hospitals). Nutrition and morbidity studies were among the additional indicators recommended for study.

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Josef Brožek

INFORMATION FOR CONTRIBUTORS

HUMAN BIOLOGY, a record of research, is a quarterly journal devoted to the subject its name implies. It publishes original articles dealing with human genetics, development, growth, reproduction and aging, functional anatomy, bio-anthropology, human ecology, population dynamics, etc.

The original manuscripts, typed double-spaced on one side of 8½ x 11 inch paper in complete and finished form precisely as they are to appear, should be sent—packed flat—to the editor, Dr. Gabriel Lasker, Wayne State University College of Medicine, 1401 Rivard Street, Detroit 7, Michigan. It is desirable that a second copy be included in order to expedite prompt consideration, and that an additional carbon copy and copies of all illustrations should be retained by the author.

Where possible, illustrations should be made for reproduction as line engravings, hence drawn in India ink as boldly as possible. Lettering must remain legible after figures are reduced to fit on 4¼ x 6¾ inch pages (letters 1½ to 2 mm high after reduction are adequate). Figures should be numbered consecutively with Arabic numerals. Tables should be numbered independently from 1 up, and both figures and tables should have captions. When the amount of tabular and illustrative material is judged to be excessive or unusually expensive, authors may be requested to pay the excess cost. Galley proofs will be sent to the author and must be returned promptly; when the author makes substantial departures from copy, the cost will be charged to him. Three copies of an abstract (not to exceed 225 words) should be submitted when the proof is returned. One will be sent to the editor of *Biological Abstracts*.

References in the text to the literature should be made by using the author's name followed by the year of publication: "Studies of Garn and Saalberg (1953) . . ." At the end of the article, under the heading "LITERATURE CITED," full references with titles and inclusive page references should appear (double spaced) in alphabetical order of the authors' names: "GARN, S. M. AND J. H. SAALBERG 1953 Sex and age differences in the composition of the adult leg. Human Biol., *25:* 144-153."

Authors will receive 25 free reprints. Orders for additional reprints should be placed at the time proof is returned. It will not be possible to accept orders for reprints after the proof has been returned to the press.

HUMAN BIOLOGY

a record of research

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Manuscripts and communications regarding editorial matters should be addressed to Editor, Human Biology, Department of Anatomy, Wayne State University College of Medicine, 1401 Rivard St., Detroit 7, Michigan. Books for review should be sent to Dr. Josef Brožek, Department of Psychology, Lehigh University, Bethlehem, Pennsylvania.

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QUANTIFICATION OF BODY WEIGHT AND CONFIGURATION FROM ANTHROPOMETRIC MEASUREMENTS *

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and

CARROLL BRODSKY

INTRODUCTION

ONE of the most difficult measurements to make in all biology, namely, the surface area of the human body, can be calculated accurately from the parameters of height (h) and weight (W), and the conversion constant K. The general form of the equation employed is $A = W^a \times h^b \times K$. Although this type of formulation appears to be universally applicable, the values for K and exponents a and b were obtained from measurements on less than 12 individuals. This fact indicates that in healthy adults whose body form is not distorted by excessive fat, there is an organized body structure or pattern characterized by anthropometric relationships obtainable on a few individuals and applicable to many.

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* A related article, The estimation of lean body weight from "skeletal" measurements, by Albert R. Behnke, is scheduled for publication in the December issue of *Human Biology*—Ed.

In turn, body weight can be calculated with remarkable accuracy, as the data in this report will show, from the product of stature (h), a constant (K), and the squared sum of certain girth measurements and bideltoid diameter. It is possible also to divide the dimensions by a constant (k) to compute a "body radius," (R). If we employ a cylinder as a geometrical analogue of the body, then with units expressed in kilograms or decimeters,

$$W = \pi R^2 h \qquad \text{and} \qquad R = \sqrt{\frac{W}{\pi h}}$$

An additional constant (π) is introduced by the use of these formulas, but they serve to emphasize some interesting geometrical relationships, as von Schelling (1954) indicated, between body dimensions and surface area, as well as weight.

Essential and extensive supporting data for this analysis have been derived from several sources. Willoughby (1932) recorded certain envelope dimensions on a group of athletes, and also on young women, from which he computed optimal proportions of the body. The Air Force has prepared exemplary brochures summarizing data on 4,000 men (Hertzberg, *et al.*, 1954), and on young women (Daniels, Meyers, Worrall, 1953) and men trainees (Daniels, *et al.*, 1953). Earlier measurements on 25,000 Army separatees, and those of Randall *et al.* (1949) are especially valuable for the purposes of this analysis. These military investigations were broad anthropometric surveys, but the mean and percentile distribution of values recorded for the different groups are in good agreement. The techniques employed (invariably with a few exceptions) also are the same. It was possible, therefore, from the Navy sample to derive constants for the conversion of anthropometric dimensions into equivalent weights which were nearly identical to those we have derived from an analysis of data on much larger groups.

BASIC RELATIONSHIPS

The mean and median values for the anthropometric dimensions are nearly the same. In random samples, the coefficients of variation (C. V.) of a number of measurements, if stature and waist girth are excluded, fall in the range of 5 to 8. For example, from the data of Hertzberg *et al.* (1954), the C. V. for the bideltoid diameter is 5.1, and for the girths of the shoulder, 5.5; chest, 6.3; biceps, 8.3; forearm, 6.4; buttocks, 6.1; thigh, 7.8; and calf, 6.7. If an average rounded value of 6% is used, then with stature constant, the distribution of anthropometric dimensions is depicted by the lower curve in figure 1.

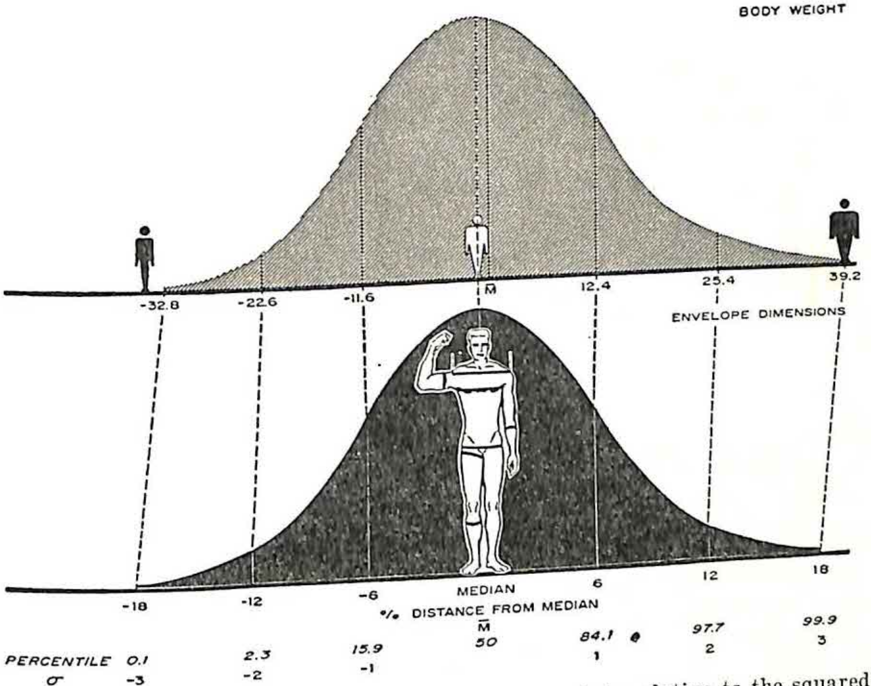

FIG. 1. Distribution of body weights (upper curve) in relation to the squared values of anthropometric dimensions (lower curve) for a given stature. The relationship between correlates in the upper and lower curves is indicated by the connecting lines. If the sum of an individual's dimensional values lies at a distance of 6% to the right of the median dimensional value (lower curve), his body weight value will lie at a distance of 12.4% to the right of the median weight value (upper curve).

The C. V. for body weights is about twice that computed for the anthropometric dimensions, *e. g.*, from the data of Hertzberg *et al.*, C. V.

(Weight) = 12.74%. The distribution of body weights calculated from the squared values of the anthropometric dimensions is shown in the upper curve of figure 1. Thus, if an individual's position on the dimensional distribution curve is 6% to the right or 6% to the left of the median value (M), as the case may be, his position on the weight distribution curve will be 12.36% to the right or —11.46% to the left of the median value for body weights, respectively. This follows from the formulas,

$$(M + .06M)^2 = M^2 + .1236M^2$$

and

$$(M - .06M)^2 = M^2 - .1164M^2.$$

Intrinsic meaning of the coefficients of variation.

With respect to the relationship of variables such as weight (Y) and anthropometric dimensions (X) amenable to expression by the logarithmic formulation,

$$\log Y = \log bX + \log a,$$

the value for the slope b (provided X and Y are highly correlated), is aproximated by the ratio of C. V. (Y) to C. V. (X), Behnke (1953). For the purpose of our analysis, this ratio is not critically different from two.

Stature in the logarithmic formulation is incorporated in log (a). Thus, the logarithmic plot of the relationship between weight and anthropometric dimensions consists of a series of parallel lines each of slope log b, and separated from each other by the distance determined by the values of stature incorporated in the formula as log (a). For a formal treatment of the derivation of the exponents of (X) and (Y), reference should be made to the paper of Hechter (1959).

PROCEDURES

Subjects.

The subjects, chiefly Navy men, were selected to include extremes in weight and stature. They varied in weight from 117.5 kg (258.5 lb) to 52.4 kg (115.3 lb), and in height from 201.5 cm (79.5 in) to 161 cm (63.3 in). There were obese as well as lean individuals in the group.

Measurements.

Seven measurements (bideltoid diameter and circumferences of the chest, biceps, forearm, buttocks, thigh, and calf) were summated for the calculation of body weights. The additional circumferences (waist, ankle, wrist, and knee) were employed subsequently in the partitional anthropometric analyses.

A Hrdlicka blade anthropometer was employed to measure the bideltoid diameter and a plastic tape for the circumferences. The ends of the tape were held firmly in all measurements, but the tension was not determined. The techniques employed (Air Force) were essentially those outlined by Hertzberg *et al.* (1954), with two modifications. The chest circumference was measured at the mid-point of the normal respiratory excursion (Air Force—maximal circumference during normal breathing), and the forearm (maximal circumference) measured with arm extended, hand open (Air Force—arm at right angles, hand closed). Essential characteristics of the other measurements were: biceps firmly contracted with the arm in maximal flexion; thigh circumference at the gluteal fold; maximal calf and minimal ankle and wrist circumferences; knee, tape drawn across the middle of the patella, with the knee slightly flexed to relax the patella; the waist was measured in a plane midway between the lowest margins of the ribs laterally and the superior margins of the iliac crests. This is usually the minimal abdominal circumference. Measurements were made on both the right and left extremities and the average values recorded.

It was considered that the one diameter (bideltoid) included could be determined more accurately than the shoulder circumference. This measurement, however, calls for an anthropometer, whereas all of the other dimensions require only a tape measure. In order to simplify the anthropometric procedure, one may eliminate the bideltoid diameter, or substitute the shoulder circumference.

Estimation of body fat.

This was based primarily on determinations of whole body density (d) utilizing the helium dilution chamber technique for body volume, and tritiated water (tritium) dilution for total body water (TBW), Siri (1956). The subjects' activities were not regulated with respect to fluid and mineral intake. Hence, although the techniques were carried out with care, the data cannot be regarded as precise. Siri has computed the probable error of estimate of total body fat from his combined formula which incorporates values both for density and total body determinations as ± 2 per cent body weight. His formula for the estimation of body fat from both total body water and whole body density is:

$$\% \text{ Fat} = \frac{213.66}{\text{density}} - 77.49 \left(\% \frac{\text{water}}{W}\right) - 137.4.$$

In this equation, the constants are based on values of 0.90 for the density of body fat, and 1.100 for the density of the fat-free body mass. The mean percentage of fat for the Navy group determined by these methods was ~ 19. Body fat was also estimated from the anthropometric measurements (*A*-*B* analysis) to be described in a subsequent paragraph.

Derivation of constants.

The body radius (R) for each of the 31 subjects in the Navy group was calculated from individual body weight and stature, *i.e.*, $R(W) = \sqrt{\frac{W}{\pi h}}$. From the sum of the individual body radii, the mean body radius $\bar{R}$ for the group of 31 men was found to be 1.178 decimeters. If we divide the mean values of each anthropometric measurement,

TABLE 1

Derivation of constants ("k" values) from Navy group dimensional measurements ($\bar{D}$) and mean body radius $\bar{R}$, for the calculation of body weights from anthropometric measurements.

$N = 31$, $\bar{h}$ 17.83 (dm), $\bar{W}$ 78.3 kg, *Median Value*, $W(m)$ 77.7 kg

Mean Body Radius, $\bar{R} = \sqrt{\frac{W(m)}{\pi h}} = 1.178$ dm

DIMENSIONS ($\bar{D}$)	MEAN VALUES (cm)	"k" VALUES ($D \div \bar{R}$)
Diameter		
Bideltoid	47.1	40.0
Circumferences		
Chest	95.9	81.4
Biceps	32.0	27.2
Forearm	27.9	23.7
Buttocks	97.5	82.8
Thigh	57.1	48.5
Calf	37.4	31.7
	ΣD 394.9	Σk 335.3
Additional Measurements:		
Circumferences		
Wrist	17.2	14.6
Knee	38.2	32.5
Ankle	22.5	19.1
Waist	83.4	70.8

(D(1), D(2) etc.) by $\bar{R}$ (1.178 dm), we obtain the values of the conversion constants ($k(1)$, $k(2)$ etc.) for each dimension listed in table 1. The summated conversion constant (k) represents the sum of the constants for each dimension. The sum of the mean values of the anthropometric dimensions is 394.9 cm. This value divided by $\bar{R}$ also gives the value of the summated conversion constant k, and this amounts to 335.3.

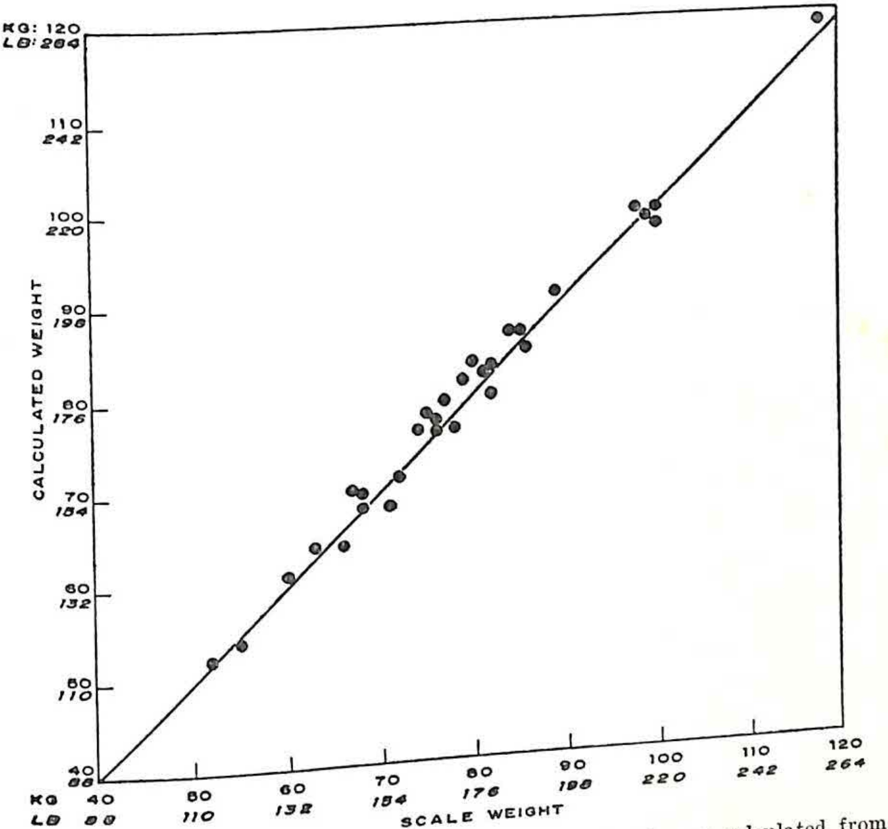

FIG. 2. Comparison of scale weights and weights of 31 men calculated from 7 anthropometric measurements (D) and stature (h). W (calc.) $= \pi R^2 h$, R is the body "radius" from $D \div$ by a constant (k).

RESULTS

Mean values for body weight and the anthropometric dimensions are shown in table 2, and for comparison, similar data on other groups are included. Garrison (1957) made an independent study of submarine personnel and employed techniques identical to those utilized in our study.

TABLE 2

Mean values of anthropometric dimensions (in cm) and body weights on young men

	STATURE dm	WEIGHT kg	BIDELTOID DIAMETER	CHEST	BICEPS	FOREARM CIRCUMFERENCES	BUTTOCKS CIRCUMFERENCES	THIGH	CALF	WAIST
Navy ($N = 31$)	17.83	78.3	47.1	95.9	32.0	27.9	97.4	57.1	37.4	83.4
C. V.	(4.9)	(18.1)	(6.5)	(8.2)	(10.2)	(4.6)	(7.8)	(9.6)	(6.7)	(11.8)
Navy ($N = 44$)	17.63	75.1	44.0	95.3	33.4	28.2	96.9	55.7	37.2	81.9
Air Force ($N = 4000$)	17.55	74.4	45.4	98.6 [1]	32.5	29.2 [2]	96.0	56.9	36.6	81.4
C. V.	(3.5)	(12.7)	(5.1)	(6.3)	(8.3)	(6.4)	(6.1)	(7.8)	(6.7)	(9.4)
Athletes ($N = 50$)	17.53	76.2	49.1	104.9	37.6	31.5	95.3	56.6	37.9	78.7

Sources: Navy ($N = 31$) Behnke *et al.*, this report; ***Navy*** **($N = 44$)** Garrison (1957); Air Force, Hertzberg *et al.* (1954); Athletes, Willoughby (1932).

[1] Maximal excursion during normal breathing.

[2] Arm flexed 90°, hand closed in a fist.

TABLE 3

Body weights on 12 of 31 subjects calculated from anthropometric dimensions (D) converted to a "body radius" (R). Estimates of percentage body fat were made from body density and water determinations, and from anthropometric measurements (A — B).

SUBJECT	AGE	STATURE (h) dm	ΣD cm	R ($\Sigma D \div 335.3$) dm	WEIGHT ($\pi R^2 h$) km	WEIGHT (Obs.) kg	BODY DENSITY	BODY WATER kg	WATER (% W)	% FAT[1]	% FAT ($A - B$)
1	34	20.15	460.2	1.373	119.3	117.5	1.020	54.6	46.4	36.2	28.3
2	52	17.88	445.1	1.328	99.1	100.0	1.033	49.6	49.6	31.0	21.2
22	48	17.75	442.7	1.320	97.1	100.0	1.020	57.6	57.6	27.5	27.4
4	31	18.03	439.9	1.312	97.5	98.9	1.014	48.3	48.8	35.5	34.4
13	39	18.80	433.2	1.292	98.6	98.4	1.040	53.5	54.4	25.8	20.7
21	22	17.65	400.9	1.196	79.3	77.0	1.052	50.6	65.7	14.8	17.3
24	31	19.35	398.9	1.190	86.1	83.9	1.047	48.3	57.6	19.2	12.2
20	31	17.73	389.4	1.161	75.1	75.9	1.053	48.0	63.2	16.5	18.7
3	21	17.93	369.3	1.101	68.3	67.7	1.083	45.3	66.9	8.1	15.0
27	20	16.20	364.8	1.087	60.2	61.1	1.063	36.3	59.4	17.6	15.4
17	20	16.10	346.9	1.034	54.1	54.8	1.081	34.3	62.6	11.8	3.7
28	23	17.18	335.2	1.000	54.0	52.4	1.074	32.4	61.8	13.6	8.7
$\bar{M}$ ($N = 31$)	29	17.83	395.0	1.178	78.5	78.3	1.051	46.0	58.6	19.5	18.8
C.V.		4.9	7.75			18.1		14.0			

[1] $\% \text{ Fat} = \dfrac{213.66}{\text{density}} - 77.49 \left(\dfrac{\%\text{ water}}{W}\right) - 137.4$ (Siri equation).

Representative data on the 31 subjects are given in table 3. All values for the 31 subjects are plotted in figure 2. The correlation between calculated and scale weights was found to be nearly unity, and the standard error of estimate was ± 1.64 kg (2.1% $\bar{W}$). Variation in the amounts of body fat in the range of 8 to 36% did not affect the accuracy of the estimations of weight.

Variability of intra-individual measurements.

The Standard Deviation of the differences between the body radius calculated from any single dimension, and the body radius computed for any given individual from the summated dimensions is shown by the following (±) values: bideltoid diameter (2.75), and the girths of the chest (2.68), biceps (4.82), forearm (3.35), buttocks (2.03), thigh (3.66), and calf (3.68). It is observed that the circumference of the buttocks is the best single dimension, together with stature, for the estimation of body weight.

Estimation of body fat from anthropometric data.

Measurements (A) of the body trunk, which harbor the large subcutaneous and intra-abodminal fat depots, may be compared with those of the extremities (B). These reflect primarily muscular development and, to a lesser degree, fat deposition. For comparative purposes, we again utilize the Navy group values and equate individual values with the group "k" values (table 4). The "trunk" measurements (A) include circumferences of the chest, abdomen, buttocks and, in addition, the thighs. The constant ($k(A)$) is the sum of the "k" values (taken from table 1) for each of the dimensions in group A, and is equal to 283.5. The radius ($R(A)$) is obtained from $A \div k(A)$, and body weight ($W(A)$) computed from group "A" is equal to $\pi(R(A))^2h$. The "extremity" measurements include the circumferences of the biceps, forearm, wrist, knee, calf and ankle, as well as the bideltoid diameter.

Likewise, for group "B" (table 4) the constant $k(B)$ is the sum of the previously derived "k" values for group B dimensions. $R(B)$ and $W(B)$ are calculated in the same way as $R(A)$ and $W(A)$.

The chest circumference presents a problem as to which category, "A" or "B," it belongs, since it may enlarge either by fat accretion or as a result of special muscular exercises, such as weight lifting. In Willoughby's athletes, for example, the difference between chest and waist girth is 26.2 cm compared with a corresponding difference of

12.5 cm for the Navy group. In the athletes, the abnormally large chest girth relative to that of the waist may be attributed to unusual muscular development and not to fat deposit. Therefore, when the radius (R), computed from the chest circumference, is greater than the radius (R) derived from waist girth, as in the athletes, the chest circumference is removed from category "A" and placed in "B." This serves to decrease the value of $k(A)$ in this case to 202.1, *i. e.* $283.5 - 81.4$, and to increase $k(B)$ from 188.8 to 270.2.

The estimation of body fat from the two groups of measurements is based on the fact that the mean fat content in the Navy group is ~19 per cent of body weight, and $\bar{W}(A) = \bar{W}(B)$. In a given individual, it is presumed also that when $W(A) = W(B)$, fat constitutes 19 per cent of the individual's body weight. The amount of excess fat below or above 19 per cent body weight is obtained by the difference between $W(A)$ and $W(B)$. The estimate of the total amount of excess fat for a given individual is therefore $0.19W + W(A) - W(B)$, as shown in table 4.

In table 2, selected data are recorded for estimates of body fat by means of the partitional analysis. The correlation ($r = 0.80$) obtained on the 31 men between body density values and the estimate of excess fat from the A-B analysis is perhaps no better at this time than the estimate of fat from skinfold measurements. However, it encourages further examination and simplification with a view of using several abdominal girths at different levels for comparison with the body radius. Area measurements from somatotype photographs of the lateral and posterior aspects of the trunk of the body would appear also to serve the same purpose.

Delineation of body pattern.

The deviations of single radii ($R(s)$) from the body radius (R) can be expressed in terms of percentages, *i. e.*, $R(s) \div R \times 100$, as shown in figure 3. A deviation of 6 per cent from R is equal approximately to 1σ. The occasional extreme inter-individual variability of certain dimensions is apparent from the data in table 4, and from figure 3. Thus, the radius calculated from the circumference of the biceps (1.518 dm) of subject 2 is greater by 13.8% than the subject's body radius. By contrast, the bicipetal radius of subject 4 (1.199 dm) is 8% smaller than his body radius and hence appears as a negative deviation on the abscissa scale of figure 3. Likewise, there is a big difference in the respective radii computed from the wrist measurements of the two subjects. Noteworthy is the fact that both of these men have approximately the same body radius, weight, stature and content of total body water and fat.

TABLE 4

Estimation of body fat from anthropometric measurements in two subjects and in athletes (Willoughby, 1932) from the difference in body weights calculated from trunk—thigh (A) and bideltoid—extremity (B) dimensions

“k” VALUES	DIMENSIONS (D)		SUBJECT 2		SUBJECT 4		ATHLETES	
		h (dm)	17.88		18.02		17.53	
		W (dg)	100.0		98.9		76.2	
		R (dm)	1.334		1.322		1.176	
			D cm	R dm	D cm	R dm	D cm	R dm
“A” GROUP							$k(A) = 202.1$	
81.4	Chest		108.0	1.327	109.6	1.346	—	—
82.8	Buttocks		108.0	1.304	108.0	1.304	95.3	1.151
48.5	Thigh		64.2	1.324	67.7	1.400	56.6	1.167
70.8	Abdomen		100.7	1.415	99.1	1.400	78.7	1.112
$k(A)$ 283.5			380.4		384.4		230.6	
$R(A) = A \div k(A)$				1.342		1.356		1.141
$W(A)$			101.1		104.1		71.7	
“B” GROUP							$k(B) = 270.2$	
	Chest		—		—		104.9	1.288
40.0	Bideltoid		51.3	1.283	51.5	1.288	49.1	1.288
27.2	Biceps		41.3	1.518	32.6	1.199	37.6	1.382
23.7	Forearm		31.9	1.346	27.3	1.152	31.5	1.329
14.6	Wrist		19.4	1.329	16.6	1.137	18.0	1.233
32.5	Knee		43.3	1.332	40.7	1.252	37.5	1.154
31.7	Calf		40.4	1.274	43.2	1.363	37.9	1.196
19.1	Ankle		23.8	1.246	25.6	1.340	22.4	1.173
$k(B)$ 188.8			251.4		237.5		338.9	
$R(B) = B \div k(B)$				1.332		1.258		1.254
$W(B)$			99.6		89.6		86.6	
$W(A) - W(B)$			+1.5		+14.5		—14.9	
19% Scale Weight			19.0		18.8		14.5	
Excess Fat (kg)			20.5		33.3		—00.4	

REFERENCE MAN AND WOMAN

From various sources listed in tables 5 and 6, it has been possible to cull comparable data in which anthropometric measurements have been made in a consistently uniform maner. Some deviation in measurement procedure in the different groups may be noted with reference to the chest (*e.g.*, girth recorded either during the maximal or mid-phase of the normal respiratory excursion) and to the forearm (arm extended with hand open, or flexed 90° with the hand doubled into a fist). Since errors of measurement are raised to the second power in the calculations, it is mandatory in future studies to define precisely not only anatomical landmarks for such girths as shoulder and chest circumferences, but to employ a tape measure which registers also the desired tension which is applied to the tape. Despite these shortcomings it is observed that the "*k*" values recorded in the tables are remarkably consistent and

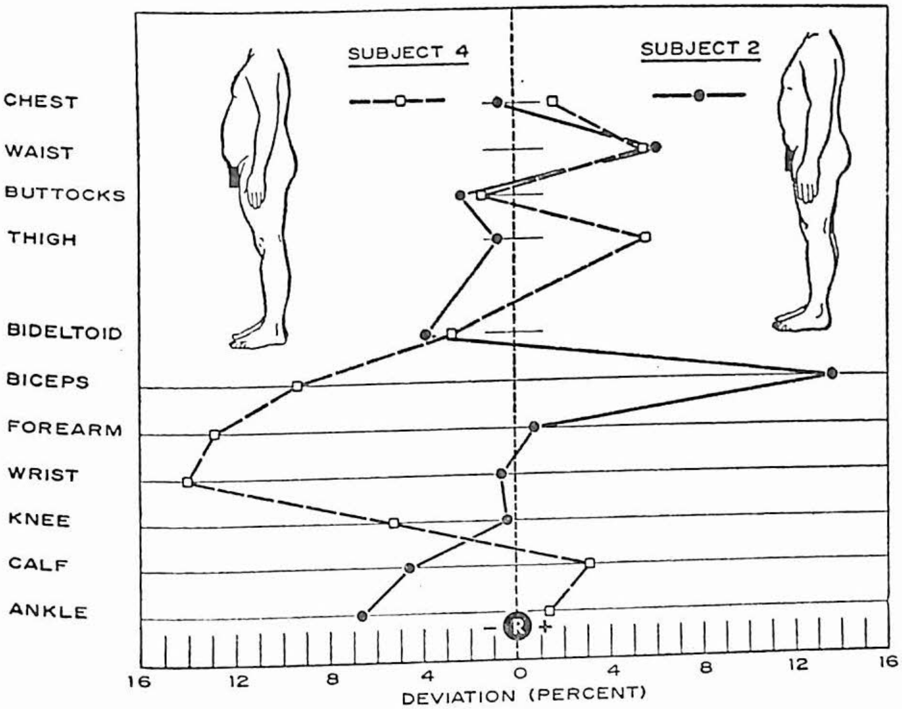

FIG. 3. Body patterns showing percentage deviation from body radius *R* calculated for each subject from his body weight, of single anthropometric dimensions converted to their respective radii in two individuals showing marked differences in musculature, reflected mainly in arm size.

they do not deviate too far from the constants derived from the Navy data.

TABLE 5

Derivation of proportionality constants (k values) for a reference man from several sources. The values for k are obtained by dividing the mean values for each dimension by $\bar{R}$, the mean body radius. The $\bar{R}$ value for each group is derived from $\sqrt{\frac{W}{\pi h}}$, W represents median weight and h, mean stature. The mean values for each dimension can be calculated from the product $k\bar{R}$.

	ARMY [1]	NAVY	AIR FORCE [2]		WILLOUGHBY [3]	REFERENCE MAN
N	25,000	31	4,000	3,000	52	
$\bar{R}$ (dm)	1.128	1.178 [a]	1.155	1.100	1.171	1.126
h (dm)	17.39	17.83	17.55	17.41	17.53	17.40
Median W (kg)	69.5	77.7 [b]	73.6	66.1	75.5 [b]	69.3
Mean W	70.4	78.3	74.4	66.9	76.2	70.0
DIMENSIONS			k Values ($D_{(cm)} \div \bar{R}$)			
Diameter						
Bideltoid	40.4	40.0	39.3	—	41.9	40.0
Circumferences						
Shoulders	—	—	(99.5)	(98.2)	—	—
Chest	81.9	81.4	85.4 [c]	82.1	89.6	82.0
Biceps	—	27.2	28.1	28.1	32.1	28.1
Forearm	—	23.7 [d]	25.3 [e]	24.6 [e]	26.9 [d]	24.6 [e]
Buttocks	82.4	82.8	83.1	83.9	81.4	82.8
Thigh	48.3	48.5	49.3	49.4	48.3	48.8
Calf	31.7	31.7	31.7	32.0	32.4	31.7
$\bar{M}$		335.3	342.2		352.6	338.0
Waist	69.0	70.8	70.5	70.0	67.2	69.0

Sources: [1] Quartermaster General Report No. 180 (1951); [2] $N = 4{,}000$, Hertzberg *et al.* (1954), $N = 3{,}000$, Daniels *et al.* (1953); [3] Willoughby (1932).

[a] For the Navy group, $\bar{R}$ was the mean of 31 individual R values calculated from $\sqrt{\frac{W}{\pi h}}$.

[b] Calculated from $\pi \bar{R}^2 h$.

[c] Maximal circumference during normal breathing.

[d] Arm extended, hand open.

[e] Arm flexed 90°, hand closed.

TABLE 6

Derivation of proportionality constants (k values) for a reference woman from several sources (see caption, table 5)

		USDA			
	WAF TRAINEES [1]	WOMEN [2]	GIRLS,[3] AGE 17	WILLOUGHBY [4]	REFERENCE WOMAN
N	852	10,041	2771	20	—
R (dm)	1.042	1.073	1.027	1.045	1.047
$\bar{h}$ (dm)	16.27	16.05	16.11	16.27	16.38
W (kg) (median)	55.5	58.5	—	—	56.4
$\bar{W}$ (kg)	56.0	60.7	53.4	55.8	56.8
		$k\,(D_{(cm)} \div \bar{R})$ values			
Circumferences					
Shoulders	92.1	—	—	—	92.1
Chest [a]	78.8	—	—	—	78.8
Upper arm [b]	26.8	26.9	24.2	—	26.8
Biceps (flexed)	25.6	—	—	27.0	25.6
Forearm	23.1	23.1	—	23.1	23.1 [c]
Buttocks	90.0	91.9	89.9	90.0	90.0
Thigh	53.3	52.6	52.1	52.7	52.6
Calf	32.7	31.8	32.4	32.5	32.5
Waist	64.5	69.0	62.9	63.0	

Sources: [1] Daniels *et al.* (1953); [2] O'Brien *et al.* (1941); [3] O'Brien *et al.* (1941); [4] Willoughby (1932).

[a] Chest, arm scye level and above mammary glands.

[b] Axillary arm circumference.

[c] Arm flexed 90°, hand closed.

If a value of 69.3 kg (the median weight of a "reference" group), and a stature of 17.40 dm be selected for a reference man, then his body radius will be 1.126 dm. The values in table 7 for certain dimensions (D) for this man can be calculated from the product of $R \times D$(cm). Likewise, similar dimensional measurements can be calculated for a "reference" woman whose weight is 56.4 kg and stature 16.38 dm.

Male and female complementary measurements.

In table 7 it is seen that the sum of the "k" values for a reference man and woman are nearly equal, although the constants for a given dimension may differ considerably in the two sexes. Thus, the greater shoulder circumference of the man relative to his size is indicated by the

TABLE 7

Complementary relationships between anthropometric dimensions for a reference man and woman based on the summated values of their weight conversion constants (k values).

GROUP DATA	REFERENCE MAN			REFERENCE WOMAN		
Age	22			18 to 25		
Stature $\bar{h}$ (dm)	17.40 (68.5 in)			16.38 (64.5 in)		
Weight (kg)						
Median	69.3 (152.5 lb)			56.4 (124 lb)		
Mean	70.0 (154.0 lb)			56.8 (125 lb)		
Body Radius ($\bar{R}$) $= \sqrt{\frac{W\ (\text{median})}{\pi \bar{h}}}$	1.126 (dm)			1.047 (dm)		
INDIVIDUAL DATA DIMENSIONS (D)	D (cm)	k VALUES ($D \div \bar{R}$)		D (cm)	k VALUES ($D \div \bar{R}$)	
Shoulders	111.5	99	181.8	96.4	92.1	182.1
Buttocks	93.2	82.8		94.2	90.0	
Biceps	31.6	28.1	133.2	26.8	25.6	133.8
Forearm	27.7	24.6		24.2	23.1	
Thigh	54.9	48.8		55.1	52.6	
Calf	35.7	31.7		34.0	32.5	
	Σ 354.6	315.0		Σ 330.7	315.9	
Adidtional circumferences						
Chest (arm scye level)	96.8	86		82.5	78.8	
Waist	77.7	69		66.0	63.0	

"k" value of 99, compared with the corresponding value of 92.1 for the woman. On the other hand, the relatively larger circumference of the hips of the woman compared with that of the man is evident from the respective "k" values of 90 and 82.8. The sum of the "k" values, however, for these two dimensions is equal for the man and the woman. We have applied the term "complementary" to this relationship. Upper and lower extremity girth measurements for the two sexes are likewise complementary. These reciprocal relationships may be expressed as:

$$\overset{\text{Woman}}{\frac{k\ (\text{shoulders})}{k\ (\text{hips})}} = \overset{\text{Man}}{\frac{k\ (\text{hips})}{k\ (\text{shoulders})}}$$

$$\frac{k\begin{cases}\text{biceps}\\ \text{forearm}\end{cases}}{k\begin{cases}\text{calf}\\ \text{thigh}\end{cases}} = \frac{k\begin{cases}\text{calf}\\ \text{thigh}\end{cases}}{k\begin{cases}\text{biceps}\\ \text{forearm}\end{cases}}$$

DATA ON OTHER GROUPS

As a test of the general applicability of the anthropometric conversion constants ("*k*" values), it was of interest to analyze measurements of the circumferences of the calf on military personnel in Yugoslavia (1957) and in Russia (1883). The calf circumference was chosen primarily because it is amenable to a uniform technique of measurement despite the fact that intra-individual variation with respect to other dimensions is large. In this regard it approaches the biceps in that it can be altered by certain types of muscular exercise. Since some concern at times has been expressed in regard to possible disuse atrophy of the lower extremities in a mechanized American population, it was of interest further to examine data on groups of men who might be expected to have unusually well developed lower extremities.

In the Yugoslavian data, only mean values were available on commissioned and non-commissioned Army officers; weight (69.0 kg), stature (17.20 dm), and calf girth (25.84 cm). If this calf girth is then divided by the conversion constant (31.7) for the reference man, the body radius computed from this single dimension is 1.1305 dm, and caluculated body weight from $\pi R^2 h = 69.1$ kg.

The Russian data were obtained from individual measurements recorded on soldiers by Kupriyanoff in 1883. The "*k*" values for the chest and calf girths of the reference man were used to predict body weight from these combined anthropometric values (table 8). Kupriyanoff also computed a body radius. Some principles in his paper, as well as in the reports of Willoughby (1932) and Turner (1943), are woven into the analyses of this paper.

GEOMETRICAL REPRESENTATION OF THE BODY

We have compared the body to a cylinder and established the reality of a body radius, *R*, in terms of anthropometric-weight (volume) relationships. The question arises, what is the surface area of the body, of specified radius *R*, and stature, in terms of the cylinder analogue. As previously stated, surface area is extremely tedious to measure, but it can be calculated rather well from the DuBois general formula (DuBois and DuBois, 1915):

$$A = W^a \times h \times K.$$

As commonly employed, the value of the exponent *a* is 0.425, and *b*, 0.725. However, "the formula, $A = W^{\cdot 5} \times H^{\cdot 5} \times 167.2$ was almost as "good," *i. e.* the surface area of the body is proportional to the square root of the product of W and H. If this is so, one may derive from the

TABLE 8

Calculated body weights of Russian soldiers from anthropometric measurements made in 1883[1] *based on constants derived from similar data on American military personnel in 1957.*

	HEIGHT (dm) (1)	CIRCUMFERENCES Chest (cm) (2)	Calf (cm) (3)	CALCULATED WEIGHT (kg)[2] from (1), (2) (4)	(1), (3) (5)	AVERAGE (4), (5) (6)	SCALE WEIGHT (kg) (7)
	17.76	95.4	37.2	75.6	77.0	76.3	74.1
	17.37	91.0	35.5	67.2	68.5	67.9	67.6
	16.81	93.2	35.5	68.3	66.3	67.3	68.8
	17.32	94.3	37.2	72.0	74.8	73.4	73.7
	16.70	93.2	35.5	64.0	65.2	64.6	61.4
	15.37	86.6	33.8	53.9	54.2	54.1	57.7
	15.54	88.5	33.3	53.1	53.9	53.5	56.1
	15.65	86.6	34.4	54.7	57.9	55.3	59.0
	15.81	84.4	34.9	52.1	60.2	56.1	56.9
	15.87	84.4	34.4	52.8	58.7	56.0	56.5
	15.98	86.6	33.8	56.0	57.0	56.5	57.3
	16.26	88.8	34.4	61.4	60.2	60.8	58.1
	16.37	89.9	33.8	61.8	58.5	60.2	61.4
	16.59	89.9	35.5	62.6	65.4	64.0	64.7
	16.81	94.3	33.3	69.8	58.3	64.0	64.7
	17.09	90.5	37.7	65.4	75.9	70.7	70.4
	17.20	93.8	38.8	70.7	81.0	76.4	79.4
	17.59	94.3	36.6	73.1	73.6	73.4	77.8
	16.93	91.0	36.6	65.5	70.9	68.2	67.2
	17.15	86.6	34.4	60.1	63.4	61.8	65.5
	17.76	98.8	40.0	81.0	88.8	84.9	85.2
M	16.66	90.6	35.6	63.9	66.1	65.0	65.9
C.V. %	4.8	4.2	5.9	—	—	13.1	12.2

[1] Source: Kupriyanoff, 1890-91.

[2] Calc. $W = \pi R^2 h$, $R\,(\mathrm{dm}) = \dfrac{\text{Chest C.}}{82.0}$ or $\dfrac{\text{Calf C.}}{31.7}$.

analysis of data furnished by one of the authors (A. R. B.) to von Schelling (1954) an expression for surface area in terms of R, the body radius, and h, stature, such that

$$A = 3\pi Rh = 2R \times 1.5h$$

Thus, the surface area of the body whose radius R is equal to a cylinder of radius r, can be represented by the cylindrical area $(2\pi r \times 1.5h)$ of a second cylinder of the same radius as the first cylinder, but 1.5 times greater in height, figure 4.

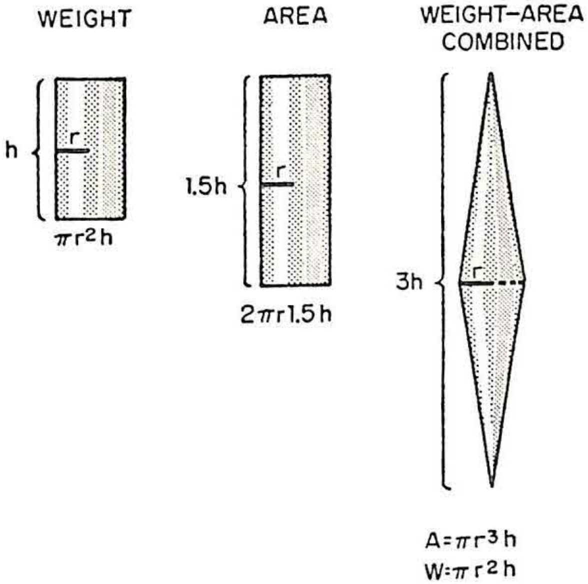

FIG. 4. Geometrical analogues of the body. Equivalent body weight for a given stature (h) may be represented by the volume of a cylinder with radius r and body area by the cylindrical area of a second cylinder (radius r), but 1.5 times longer than the first cylinder. The double cones (von Schelling) incorporate the body weight-area relationship in the same model. The long axis of this model, however, is 3 times body stature.

It can be shown also that both volume and surface area relationships characteristic of the human body can be represented by von Schelling's double cone model, figure 4. Data for surface area of large individuals were calculated on the basis of $A = 3\pi Rh$, and they are compared with values calculated by Sendroy and Cecchini (1954) from their formula and the formulas of DuBois and Boyd, table 9.

COMMENTS

The constant, π, in the formulas, $W = \pi R^2 h$ and $A = 3\pi Rh$, can be eliminated by dividing the "k" values in the tables by $\sqrt{\pi}$ and π respectively. Thus, since $\frac{D}{k}$, and $k = 335.3$ for the Navy group (table 1),

$k \div \sqrt{\pi} = 189.2$, so that

$$W(\text{kg}) = \left(\frac{D}{189.2}\right)^2 h = 2.794 \times 10^{-5} D^2 h,\ D \text{ in cm},\ h \text{ in dm}.$$

Likewise, surface area (sq. dm) $= 3\pi Rh = 2.81Dh$, SA in sq. cm.

From the buttocks circumference $(D(b))$:

Male: $W(\text{kg}) = 4.58 \times 10^{-4} D^2(b)h$

Surface area (sq. cm) $= 1.138D(b)h$.

Female: $W(\text{kg}) = 3.88 \times 10^{-4} D^2(b)h$

Surface area (sq. cm) $= 1.047D(b)h$.

TABLE 9

Surface area estimated for individuals of large stature from the formula, $A = 3\pi Rh$, and from other formulas by Sendroy and Cecchini (1954)

SUBJECT	H (cm)	W (kg)	SURFACE AREA (m^2)			
			DuBois	$3\pi Rh$ [1]	Boyd	Sendroy *et al.*
A	197.5	115.5	2.50	2.54	2.51	2.47
B	208.3	109.1	2.54	2.53	2.47	2.45
C	200.7	109.1	2.47	2.49	2.45	2.41
E	261.6	232.3	4.12	4.14	3.97	4.24
I	203.2	90.0	2.29	2.27	2.21	2.20
K	200.7	84.1	2.21	2.18	2.12	2.15
O(a)	182.9	80.8	2.03	2.04	2.02	2.00
O(c)	259.1	197.4	3.82	3.81	3.64	3.84
O(e)	268.0	222.8	4.13	4.11	3.92	4.23

[1] Values in sq. decimeters divided by 100.

SUMMARY

1. In a group of 31 Navy men it was shown that body weights could be calculated accurately from circumferential anthropometric measurements (D) mainly, and stature (h).

2. D divided by a constant $(k) = R$, the "radius" of the body, and $W = \pi R^2 h$.

3. Surface area in turn is equal to $3\pi Rh$.

4. The weight, surface area formulas may be rearranged to eliminate the constant, π, so that

$$W = D^2hk \text{ and } A = Dkh.$$

5. The number of measurements required to estimate weight and surface area may be reduced to two (girth of hips and stature) without introducing appreciable error.

6. Consistent anthropometric data were obtained on both men and women and from the dimensional conversion constants ("k" values) the following complementary relationships were revealed:

$$\begin{array}{ccc} \text{Woman} & & \text{Man} \\ \dfrac{k\ (\text{shoulders})}{k\ (\text{hips})} & = & \dfrac{k\ (\text{hips})}{k\ (\text{shoulders})} \\ \hline \dfrac{k\left\{\begin{matrix}\text{biceps}\\ \text{forearm}\end{matrix}\right.}{k\left\{\begin{matrix}\text{calf}\\ \text{thigh}\end{matrix}\right.} & = & \dfrac{k\left\{\begin{matrix}\text{calf}\\ \text{thigh}\end{matrix}\right.}{k\left\{\begin{matrix}\text{biceps}\\ \text{forearm}\end{matrix}\right.} \end{array}$$

7. Anthropometric measurements translated into their weight equivalents make possible a representation of body configuration.

8. Constants derived from the Navy data for the estimation of body weight from calf and chest circumferences were applicable to similar data obtained on Russian soldiers in 1883.

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THE RELATIONSHIP BETWEEN WEIGHT AND SOME ANTHROPOMETRIC MEASUREMENTS IN ADULT MALES

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THERE have been a number of attempts to derive a functional relationship between weight (total or lean body) and a set of concomitant anthropometric measurements. The difficulties involved in determining the lean body weight makes this approach especially useful. Often the body weight has been expressed as a linear combination of the anthropometric variables (Brožek, 1956; Turner, 1943; McCloy, 1936), other authors (Massler, 1945; Günther, 1948), using a dimensional analysis, have expressed the body weight as being proportional to the product of two variables each raised to an appropriate exponent. Behnke, et al., (1959) have used a combination of these methods by assuming that the weight is proportional to the product of stature and the square of the sum of a number of anthropometric measurements. This paper derives a simple dimensional relationship and indicates the relative importance of the anthropometric variables which were used. The relationship will be indicated for the total body weight first and later in the paper for the lean body weight. The analysis is based on the measurements made by Behnke *et al.*, (1959) and of Behnke (1959) on a sample of 31 male individuals (age range—20 to 52 years) they have kindly furnished for this purpose. Reference is made to their papers for the discussion of the procedures and techniques involved in taking the measurements.

PREDICTION OF TOTAL BODY WEIGHT

The mean and standard deviation for the variables used in the regression analysis are given in table 1. With the exception of stature and the bideltoid measurement, all concomitant variables are circumferential.

Presuming the body weight to be proportional to body volume, a formula of the form

$$W = k_1 m_1^{a_1} m_2^{a_2} \ . \ . \ . \ m_9^{a_9} \tag{1}$$

$$W = 466 \times 10^{-7}\ C \cdot B \cdot S \tag{4}$$

where $C = m_2 =$ Chest circumference
$B = m_6 =$ Buttocks circumference
$S = m_9 =$ Stature.

TABLE 4

Test of the hypothesis of $a_2 = a_6 = a_9 = 1$

VARIATION	DEGREES OF FREEDOM	MEAN SQUARE	F
Deviation from $a_2 = a_6 = a_9 = 1$	3	0.00014	1.0 (not significant)
Residual	27	0.00014	
Total	30		

It is of interest to determine how much precision is sacrificed if only two measurements, stature and one other, are used. Table 5 gives the estimate of the coefficients and coefficient of variation.

TABLE 5

Estimate of the coefficients when two variables are used

STATURE AND	BUTTOCKS	XYPHOID	CHEST	CALF	THIGH	FOREARM	BIDELTOID
0.90	1.91						
1.15		1.73					
1.04			1.80				
1.06				1.94			
0.70					1.59		
0.96						1.92	
1.00							2.07
CV	4.0%	6.1%	5.0%	8.8%	4.8%	9.0%	8.1%

None of the coefficients is significantly different from its closest integer, indicating that a simple formula of the form

$$W = k_1 m_i^2 m_9 \tag{5}$$

where m_9 is the stature and m_i is the buttocks, chest, calf, thigh, forearm, bideltoid or xyphoid, could be used as a prediction formula. However, the variability for any combination, as shown in the last line of table 5, is significantly increased as compared to the preceding expression, formula (4), which utilizes three linear dimensions.

PREDICTION OF LEAN BODY WEIGHT

For each of the 31 individuals, determinations of the body density and total body water were made (Siri, 1957). From these determinations, an estimate of the lean body weight was calculated from a modified form of the equation given by Siri (1956). Reference is made to the papers of Behnke (1959), and Siri (1956) for the discussion of the applicability and validity of the methods. Here it suffices to note that there are a number of uncertainties involved in the method of calculating the lean body weight. The reliability of the method as expressed as a coefficient of variation is roughly estimated as 2–3% (Siri, 1956).

TABLE 6

Mean and standard deviation of the skeletal diameters

	MEAN	STANDARD DEVIATION
Lean Body Weight (kg)	63.6	8.8
Biacromial (cm)	40.6	1.9
Chest (cm)	29.9	1.9
Bi-iliac (cm)	29.4	2.3
Bitrochanteric (cm)	32.8	2.3
Wrist (cm)	5.6	0.4
Ankle (cm)	6.9	0.3
Knee (cm)	9.2	0.7
Stature (cm)	178.3	8.7

As in the preceding section on total body weight, it is assumed that a general formula of the form

$$L = c_1 t_1^{\beta_1} t_2^{\beta_2} \cdot \cdot \cdot t_8^{\beta_8} \tag{6}$$

where $L =$ lean body weight, t_i is the i^{th} skeletal measurement, would adequately describe the relationship between lean body weight and a set of skeletal diameters. The skeletal diameters that were used in the analysis, along with their mean and standard deviation are shown in table 6. The estimate of the β coefficients are given in table 7.

It is seen in table 7 that some of the skeletal variables are of little or no importance for the prediction of lean body weight. When these variables are eliminated, there is left a formula based on the product of three linear dimensions. See table 8 for the estimate of the coefficients.

TABLE 7

Estimates of the coefficients using all of the skeletal diameters

SKELETAL DIAMETER	COEFFICIENT	ESTIMATE ± S. E
Biacromial	β_1	—0.04 ± 0.58
Chest	β_2	0.72 ± 0.47
Bi-iliac	β_3	—0.34 ± 0.47
Bitrochanteric	β_4	0.72 ± 0.50
Wrist	β_5	0.39 ± 0.28
Ankle	β_6	—0.18 ± 0.46
Knee	β_7	—0.08 ± 0.30
Stature	β_8	1.17 ± 0.38
R		0.85
CV		8.1%

TABLE 8

Estimate of the coefficients of the reduced equation

SKELETAL DIAMETER	COEFFICIENT	ESTIMATE ± S. E.
Chest	β_2	0.75 ± 0.33
Wrist	β_5	0.43 ± 0.33
Stature	β_8	1.18 ± 0.34
R		0.84
CV		7.8%

Thus, the formula for predicting lean body weight can be written

$$L = 519 \times 10^{-5} C_d^{0.75} W_d^{0.43} S^{1.18} \tag{7}$$

where C_d = Chest diameter
W_d = Wrist diameter
S = Stature

and this formula is practically as precise for predicting the lean body weight as the formula which uses all of the skeletal diameters.

On 23 of the 31 individuals, radiographic skeletal measurements were obtained. To determine whether these X-ray measurements would give a closer degree of association with lean body weight than do the skeletal diameters, a regression analysis was made. The mean and standard deviation are listed in table 9.

The multiple correlation coefficient between lean body weight and the 9 x-ray variables and stature was 0.92. Most of the variables were of little or no predictive importance, and table 10 shows the estimate of the exponents or coefficients of the reduced equation. The estimates when the skeletal diameters are used (table 8) are also shown.

TABLE 9

Mean and standard deviation of the x-ray measurements and stature

	MEAN	STANDARD DEVIATION
Lean Body Weight (kg)	64.0	8.8
Elbow (cm)	6.3	0.4
Wrist (cm)	4.9	0.4
Knee (cm)	8.8	0.5
Ankle (cm)	6.5	0.3
Proximal Tibia (cm)	8.1	0.4
Distal Tibia (cm)	5.3	0.3
Bi-iliac (cm)	27.9	2.4
Bitrochanteric (cm)	30.5	2.2
Chest (cm)	28.1	1.6
Stature (cm)	178.7	9.6

TABLE 10

Estimate of the coefficients

SKELETAL DIAMETER	ESTIMATE ± S. E.	X-RAY MEASUREMENT	ESTIMATE ± S. E.
Chest	0.75 ± 0.33	Chest	0.75 ± 0.25
Wrist	0.43 ± 0.33	Wrist	0.43 ± 0.20
Stature	1.18 ± 0.34	Stature	1.30 ± 0.29
R	0.84		0.90
CV	7.8%		6.5%

As seen in table 10, the use of x-ray measurements for predicting lean body weight is slightly better than the use of skeletal diameters. As mentioned earlier, there are a number of uncertainties involved in determining the lean body weight by use of the body density and total body water, and this might be one of the possible reasons for the failure of the sum of the exponents to add to three.

The formula for the prediction of lean body weight from the x-ray measurements is

$$L = 300 \times 10^{-5} C_x^{0.75} W_x^{0.43} S^{1.30} \tag{8}$$

where C_x = Chest x-ray

W_x = Wrist x-ray

S = Stature.

SUMMARY

Using statistical regression analysis, equations have been derived for predicting weight (either total or lean body) in adult males from body measurements. The predictive equations and the multiple correlation coefficient, R, are as follows:

a. Total body weight

$$W = 466 \times 10^{-6}\ C \cdot B \cdot S$$

where W = total body wt. in kg

C = chest circumference in cm

B = buttocks circumference in cm

S = stature in cm

and $R = 0.99$

b. Lean body weight

(i) from skeletal diameters

$$L = 519 \times 10^{-5} C_d^{0.75} W_d^{0.43} S^{1.18}$$

where L = lean body wt. in kg

C_d = chest diameter

W_d = wrist diameter

S = stature

and $R = 0.84$

(ii) from x-ray measurements

$$L = 300 \times 10^{-5} C_x^{0.75} W_x^{0.43} S^{1.30}$$

where C_x = chest x-ray

W_x = wrist x-ray

S = stature.

and $R = 0.90$

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DATA ON THE BIOLOGY OF KOREAN POPULATIONS[1]

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APART from scattered references in the economic and sociological literature there are few, if any, data available on some of the characteristics of Korean populations that are fundamental to various problems of human genetics. Information on birth and death rates as related to socio-economic groups are of potential value for considerations of gene frequencies in successive generations. Such information is at present mostly restricted to Western countries. The secondary sex-ratio is known to vary from one group to another and its supposedly unusually high value in Korea deserves checking. The frequency of twins, and their types, is known to be different in Japan and Western countries and data on other Asian groups are desirable in order to form a judgment on the generality of the differences. Finally, the topic of consanguinity is at present under intense study by various investigators for an estimate of the genetic structure of populations and individuals, and data on frequency and types of consanguinity form one of the bases for further investigations. In order to contribute to these topics, material is presented in this report on samples of the Korean population on birth and death rates, the secondary sex ratio, frequency and types of twins and frequency and types of first cousin marriages.

METHODS

The data pertain to populations living in the city of Seoul, Cheju Island, Ulnung Island and a series of 27 cities and towns with less than 100,000 inhabitants. By means of a simple printed questionnaire, the following information was obtained, mostly from middle and high school students:

1. Occupation of father,

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2. Number of family members (parents and children on March 31, 1956, according to sex and age in decades from 0 to 81+ years).

3. Numbers of births and deaths, according to sex and age within a specified period (see below),

4. Numbers of twin pairs and sex of twins,

5. Numbers of first cousin marriages of brothers (during the period from August 1, 1953 to July 31, 1956) and relation of wife to husband through father's or mother's brother or sister.

Two different periods were covered by the questionnaire. For the city of Seoul and Cheju Island numbers of births and deaths were determined for the period April 1, 1955 to March 31, 1956 and for the series of 27 other localities the period comprised April 1, 1956 to March 31, 1957. Information on consanguineous marriages in all cases covered the three year period stated above.

This questionnaire was given to 18,227 male and female students in middle and high schools located in Seoul and Cheju. Seventeen of these schools were in Seoul. They form a random selection from the 50 or more Seoul schools. Eleven schools were on Cheju. They were selected from the twenty schools on the islands on the basis of choosing one school from one locality only.

For the small island Ulnung, one of the authors himself visited each of the 280 houses and personally obtained answers to the questionnaire.

It should be added that information on adopted children was not included in the data.

It may be assumed that the students questioned represented not only a random sample of the school population but of the population in general since there is universal education in Korea. Evidence for this statement is found in the "Statistical Yearbook of the Republic of Korea" (1957) which gives the total population of the country as approximately 20 million and that of students as about 4 million or 20% of the population.

RESULTS

Birth and Death Rates

Table 1 contains data on births and deaths in Seoul, Cheju and Ulnung Islands. In the urban environment of Seoul the death rate is significantly higher than the birth rate. On Cheju too, the death rate is higher than the birth rate, but the difference is not significant. On

Ulnung, the birth rate greatly exceeds the death rate, the difference being highly significant in spite of the smallness of the population.

The relatively low birth rate in Seoul is not unlike that of other large cities. It is undoubtedly influenced by control of conceptions. On Cheju the population consists largely of families of fishers. Here the relatively low birth rate may be correlated with the fact that the men are absent from home for long periods during fishing expeditions.

TABLE 1

AREAS	POPULATION	NO. OF BIRTHS	RATES OF BIRTH—%	NO. OF DEATHS	RATES OF DEATH—%
Seoul	102,673	1,285	12.52 ± 0.35	1,580	15.39 ± 0.38
Cheju	21,708	357	16.45 ± 0.87	328	15.11 ± 0.83
Ulnung	1,563	81	51.82 ± 5.61	19	12.16 ± 2.77
Total	125,944	1,723	13.68 ± 0.33	1,927	15.30 ± 0.35

TABLE 2

Distribution of occupational groups in 27 cities and towns with populations less than 100,000. Group 1, predominantly mental work; group 2, approximately equal amounts of mental and physical work; group 3, predominantly physical work; group 4, others.

GROUP	NO. OF FAMILIES	♂	♀	% OF POPULATION
1	1,914	7,176	7,643	15.43
2	2,527	9,095	9,408	19.26
3	7,755	31,856	28,317	62.65
4	420	1,245	1,314	2.66
Total	12,616	49,372	46,682	100.00

Data on the sample of 27 localities, based on 12,616 families are given in tables 2 and 3. These families were classified according to the father's occupation into four groups, defined in table 2. Table 2 lists the relative frequencies of the four groups including the number of males and females in each. In table 3, birth and death rates are shown for each of the occupational groups. Apart from the very small group (4), the death rates are rather similar varying from 13.1 to 14.5. The birth rates too, are similar among themselves—again omitting group (4). They lie between 19.4 and 20.8. They thus exceed the death rates in each of the three main occupational groups. This excess is smallest for the group with mostly mental work, being 5.8. For the other two groups it is 7.1 and 6.3 respectively. The trend toward a differential reproductivity of different occupational groups obviously is not striking.

TABLE 3

Birth and death rates in four occupational groups in 27 cities and towns with populations less than 100,000.

GROUP	BIRTHS		BIRTH RATES	DEATHS		DEATH RATES
(see Table 2)	♂	♀	PER 1000	♂	♀	PER 1000
1	161	127	19.43 ± 1.13	91	111	13.63 ± 0.95
2	189	185	20.21 ± 1.03	132	111	13.13 ± 0.84
3	673	580	20.82 ± 0.58	515	358	14.51 ± 0.49
4	18	21	15.24 ± 2.42	27	15	16.41 ± 2.51
Total	1,041	913	20.34 ± 0.46	765	595	14.16 ± 0.38

TABLE 4

Sex-ratios at birth

AREAS	♂	♀	SEX-RATIO	% MASCULINITY	χ^2	P
Seoul	688	597	115.25	53.62 ± 1.39	6.44	0.01
Cheju	186	171	108.77	52.10 ± 2.64	0.63	0.4
Ulnung	42	39	107.69	51.86 ± 5.55	0.11	0.7
Total	916	807	113.50	53.16 ± 1.20	6.89	0.01

The birth and death rates reported here cannot be fully compared with those from general census data. This is so because our populations were mostly ascertained from school children, a method which leads to a distribution of ages in the population, which is different from that of populations ascertained completely.

The Sex Ratio at Birth

The sex ratio at birth, *i. e.*, the secondary sex ratio varies in different parts of the world (Ciocco, 1938). Nearly always an excess of males is apparent. In most Caucasian populations the ratio of male to female births lies between 104 : 100 and 106 : 100. For Korea the unusually high ratio of 113 : 100 has been reported. On the whole, our data agree with the earlier findings of a high secondary sex ratio (tables 4, 5). In Seoul, the ratio was 115 : 100 based on 1,285 births. On Cheju and Ulnung

Islands, it was 108.8:100 and 107:100 respectively based on smaller samples. The 1,954 births in the 27 localities again gave a very high ratio, namely 114:100. The combined data give a ratio of 113.78:100.

The cause of this high sex ratio is unknown. It is believed to be close to the true secondary sex ratio and not to be influenced to a significant degree by underreporting of female births.

There are considerable variations in the sex ratio between the different occupational groups. No clear trend can be observed. While the very high ratio of 126.8:100 in the group with mainly mental work finds its counterpart in some data on Western upper socio-economic groups, the low ratio of 102.2:100 for the mental and physical work group as compared to the higher ratio of 116:100 for the mainly physically occupied group can only be registered without any comment.

TABLE 5

Sex-ratios at birth

GROUP (See Table 2)	SEX RATIO	% MASCULINITY	χ^2	P
1	126.77	55.90 ± 2.93	4.01	0.04
2	102.16	50.53 ± 2.59	0.04	0.8
3	116.03	53.71 ± 1.41	6.90	0.01
4	85.71	46.16 ± 7.98	0.23	0.6
Total	114.02	53.27 ± 1.13	8.38	<0.01

Twins

The only Mongoloid population for which careful data on the frequencies and types of twins are available is that of Japan (Komai and Fukuoka, 1936; Inouye, 1957). In that country the frequencies of twin births are approximately 0.6–0.7% of all births. Identical twin births account for nearly two-thirds of the total twin births. These data are in contrast to those of Caucasoid populations in which the frequencies of twin birth are greater than 1% but in which the proportion of identical twin births is only approximately one-third of the total.

Our data from Korea do not refer to twin births but rather to twin pairs among the sibs of the school children who filled out the questionnaires, or to twin pairs reported from Cheju Island. In all cases the sexual composition of the twin pairs was ascertained. This made possible the application of Weinberg's differential method for determination of the proportion of identical to non-identical pairs. All populations examined were similar in suggesting that close to two-thirds of the twin pairs were monozygotic in origin (table 6). The Korean populations thus resemble closely those of Japan in their proportions of mono- to dizygotic twin pairs.

TABLE 6

The composition of twin pairs

AREAS	NUMBER OF TWIN PAIRS	NUMBER AND RATIOS OF SEXUAL COMPOSITION ♂ ♂	♂ ♀	♀ ♀	NUMBER OF IDENTICAL TWIN PAIRS	NUMBER OF FRATERNAL TWIN PAIRS
Seoul	175	80 2.86	28 1.00	67 2.39	119	56
Cheju	30	16 3.20	5 1.00	9 1.80	20	10
27 localities	113	55 2.75	20 1.00	38 1.90	73	40
Total	318	151 2.85	53 1.00	114 2.15	212	106

The sex ratio of the combined twin pairs, 126.4 : 100, is even higher than that of the secondary sex ratio in the general Korean population as reported above. Specifically, the sex ratio of twins was 116 : 100 in 175 pairs from Seoul, 160.9 : 100 in the 30 pairs in Cheju and 132.6 : 100 in the 113 pairs from the 27 localities.

Consanguinity

It was expected that consanguineous marriages would be rare in Korea since they are prohibited in Confucianism. Our data are interesting in that they show a high frequency of first cousin marriages in the three main samples of Korean populations but also significant differences among them (table 7). During the three year period 1953-1956, no less than 7.8% of all marriages in Seoul were between first cousins and 5.9% in Cheju were of this type. In the 27 localities only 1.8% of such marriages occurred. These data are more similar to rates of consanguinity in Japan than in most Western populations.

The high frequencies of cousin marriages in Seoul and Cheju as compared to the sample from 27 other localities deserve some comment.

In Seoul two factors were involved in the high frequency. For one, the population of this city has experienced the greatest influence of Western thought and thus is inclined to deviate from Confucian prescriptions. For another, housing shortages have forced many related families to live under the same roof, leading to a tendency toward marriages between relatives. In Cheju a reason for the high frequency of cousin marriages is relative isolation of this island. On the other hand, the sample of 27 localities included rural areas in which the conservative prohibition of cousin marriage was more closely obeyed. For Seoul and Cheju it is possible to present data on the frequencies of the four possible subdivisions of first cousin marriages (table 8). It can be seen that marriages of cousins who are related through the father of the husband are less than one-half as frequent as those where the relation is through the mother of the husband. Very similar deviations from randomness of the four types of cousin marriages have been described for large samples from Austria and Japan (see Morton, 1955) but not for a group of smaller—though combined, still considerable—samples from different Western populations (Macklin, 1952). The causes underlying the nonrandomness among the Korean cousin marriages have not been studied.

TABLE 7

The frequencies of cousin marriages

AREA	NO. OF MARRIAGES	NO. OF COUSIN MARRIAGES	%
Seoul	474	37	7.81
Cheju	285	17	5.96
27 localities	605	11	1.82
Total	1,364	65	4.76

TABLE 8

Types of cousin marriages

WIFE'S RELATION TO HUSBAND	SEOUL	JEJU	TOTAL
Father's Brother's Daughter	5	3	8
Father's Sister's Daughter	5	2	7
Mother's Brother's Daughter	12	8	20
Mother's Sister's Daughter	15	4	19
Total	37	17	54

SUMMARY

On the basis of questionnaires given to 18,227 middle and high school students in various localities in Korea and of answers obtained personally by one of the authors, vital statistic data were obtained on birth and death rates, the secondary sex-ratio, the number and sexual composition of twin pairs and the frequencies and types of first cousin marriages.

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THE SUN-TANNING POTENTIAL OF HUMAN SKIN

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INTRODUCTION

COLOR of the skin has been used as one of the most important criteria of race. Skin color is an inherited character, although the exact mode of inheritance is still not clearly known. Nevertheless, the color of the skin can also be influenced by various pathological conditions and environmental factors: adrenocortical deficiency and changes in the condition of the blood, such as anemia, cyanosis, jaundice, etc., and exposure to ultraviolet rays. Ultraviolet irradiation from the sun is by far the most common and important factor. It is our purpose to investigate the extent to which this environmental factor, *i.e.*, sunlight, can influence the skin color. What is the point of saturation beyond which no further darkening of the skin can take place with further ultraviolet exposure? How does the saturation point differ in people of different original color? And do members of the same racial groups respond similarly to ultraviolet irradiation? We have subjected circumscribed areas of skin of a number of individuals to ultraviolet irradiation under controlled conditions. Records of the amount of light reflected from the skin before and after this treatment may shed some light on each of these questions.

SUBJECTS AND METHODS

Fourteen adults, 22 to 66 years of age, both male and female, participated as volunteers. Eight of them are Whites, three Monogoloids and three American Negroes.

As an index of skin color we measured the reflection, from an area of skin, of light of several wave lengths. For this we used a portable photoelectric reflection meter (Photovolt Corporation, model 610. See Lasker, 1954a, 1954b and Garn, 1956). As will be seen in a diagram of the search unit (figure 1) the incident light is transmitted through a lens, then through a colored glass filter and then through a circular aperture in the measuring photocell. The distance to the skin and back to the photocell and the area of skin studied are constant because the search unit has a small round aperture which is directly in contact with the surface to be studied. As may be seen in the illustration, the incident light strikes the skin surface at a right angle but is viewed at an average angular range of about 45°. Three glass filters were used: red, green and blue.[1]

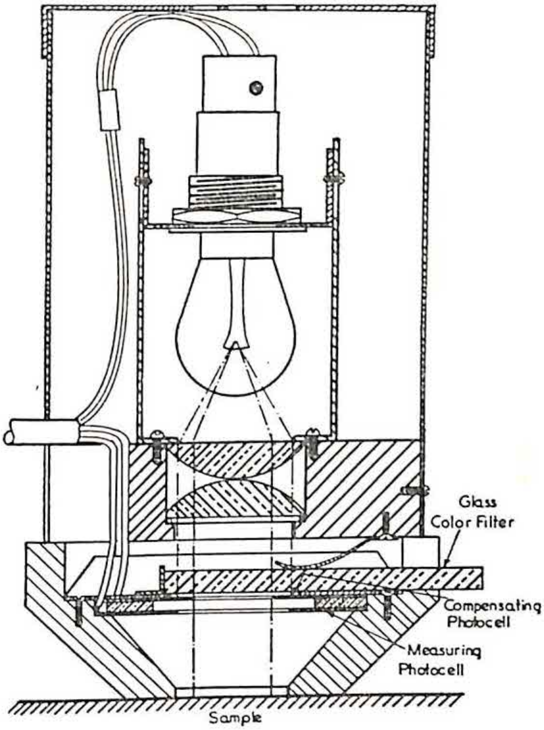

FIG 1. SEARCH UNIT OF THE PHOTOELECTRIC REFLECTION METER.

The reflection meter was standardized before each measurement against a white enamel standard which had previously been calibrated in terms of a newly prepared surface of magnesium oxide. We therefore have been able to report our results in terms of the absolute reflectance scale in which magnesium oxide is assumed to represent 100%.

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[1] The peak transmission values of the filters are 6500, 5250 and 4200 Å for the red, green and blue filters respectively. The spectral band isolated by each filter is approximately 800 Å wide. The filter curves multiplied by the emission curve of the light source give peak values shifted somewhat in the direction of longer wavelength values, but not by more than 100 to 120 Å. Furthermore this shift is counteracted at the red part of the spectrum by the opposite slant of the spectral response curve of the photocell. In other words the measurements are actually carried out at 6500, 5350 and 4300 Å.

In order to compare, in a preliminary way, the response of a few individuals of different racial origins to the process of sun-tanning, we selected an ordinary mercury-vapor sunlight lamp, type RS, produced by the General Electric Company. Although close control of the physical conditions might make the use of narrow-band sources of radiant energy desirable, our emphasis is on the biological effects. Our needs are, therefore, met to the extent that the conditions of irradiation were constant from one time to another and from one individual to another, and that the spectral energy was roughly comparable, in capacity to produce pigment, to that of sunlight.[2]

The site of the body that we chose for the experiment is over the lower part of the back. It has a large flat surface, well protected from exposure to sun (at least in Detroit during March and April when our study was under way); it lacks the uneven spotty pigmentation which sometimes occurs over the upper part of the back and shoulders; it is easily accessible, and one side can conveniently be used for irradiation and the opposite side as control. In three cases among the males there was some body hair over the lower back; this was shaved off.

During irradiation, the back of the subject was protected by an oilcloth jacket with a round hole on the right side, the only spot to be exposed to the irradiation. The center of the hole was 2½ inches (6 cm) to the right of the midline and 14 inches (36 cm) below the base of the neck. Each subject was exposed to the sunlamp once a day, except for a few days missed at weekends or when there seemed to be a risk of burning the skin. Light reflectance from the spot was measured before each exposure. Readings were also taken on the control side at the same time. While measuring the skin reflectance, the search unit was placed quite gently on the skin, as firm pressure produces a bulging of the skin and yields an incorrect value of reflectance.

The dosage of the ultraviolet irradiation is directly proportional to the exposure time, and inversely proportional to the square of the distance from the lamp to the skin. Each subject first received an empirical dose to produce a minimal perceptible erythema. The same dosage was continued for three or four days; a period which allowed the exposed skin to develop resistance to the ultraviolet rays. The dosage was then increased slightly (usually by 50%) until the skin reached minimal reflectance and did not become any less reflective despite continued daily ultraviolet irradiation. In the first two cases, we continued the irradiation for 7 additional days without any lower reading in skin reflectance being recorded, even though in the last three days the dosage was increased to 100 or 150% more than the initial dose. Based on the experience gained with the first two cases, the rest of the cases were continued with increased dosages of exposure for only three or 4 days after the reflectance became constant.

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[2] The spectral energy distribution of the RS sunlamp is approximately .01 watts between 2600 and 2800 Å, 1.70 watts between 2800 and 3200 Å, 3.40 watts between 3200 and 3800 Å and 7.60 watts in the range of visible light. The chief erythema-producing wavelengths are 2500 Å and 2967 Å while the most effective pigment-producing wavelengths are greater than 3200 Å (Koller, 1952).

FINDINGS

The skin reflectance readings of the control side provide a measure of reliability and reproducibility of the readings. There are 155 such readings for each of the three filters. The averages of the standard deviations for the 14 control sides are small, ±0.57, ±0.75 and ±0.62% for the red, green and blue filters respectively. The standard deviations are quite similar in lighter and darker individuals.

In order to compare the amount of irradiation used in different cases, the ultraviolet irradiation in this experiment is given in terms of arbitrary units. One unit is the irradiation received at a distance of 18 inches (46 cm) from the lamp in three minutes. Three of the White people needed only one unit to produce a perceptible erythemal effect, the other 5 Whites each needed 2.25 units. The three Mongoloids needed 2.25 units each, and the three Negroes needed 4 units each to produce perceptible erythema.

On the day following the initial erythema, there is a decrease in skin reflectance with the green and blue filters; no change can be observed with the red filter. The red filter shows no drop until the third or fourth day, when real tanning of the skin takes place. The exposed skin usually reached its minimum reflectance after 7 to 9 days of ultraviolet irradiation.

The decrease in reflectance of the skin in response to ultraviolet irradiation is variable even among people of the same race. Of two persons with similar initial reflectance, one may change several times as much as the other. The decrease in reflectance noted with the three different filters is shown in table 1. The Negro skins decrease little in reflectance, but since their original readings are low, their relative decrease is still considerable. Mongoloid skins decrease considerably in reflectance both absolutely and relatively. In most White skins the decrease is small, especially with the red filter. Two White individuals with black hair (one of Italian, the other of Spanish-Hungarian origin), show quite different reactions from the blond and brown-haired Whites. These two show a marked drop in skin reflectance with all three filters—even more than the three Mongoloid subjects—although their reflectance values are higher and their skin color apparently lighter than that of two of the three Mongoloids.

TABLE 1

Reflectance of the skin before and after ultraviolet irradiation.

GROUP	SUBJECT	SEX	REFLECTANCE RED FILTER INITIAL	 FINAL	 GREEN FILTER INITIAL	 FINAL	 BLUE FILTER INITIAL	 FINAL
I	1	M	68.0	62.0	46.9	29.0	40.1	25.7
	2	M	66.3	64.2	45.3	34.4	38.4	27.8
	3	M	64.7	62.5	43.2	34.1	38.7	30.9
	4	M	62.0	57.8	41.3	26.1	33.8	21.6
	5	M	61.3	57.5	40.1	32.6	33.0	25.3
	6	M	60.0	54.3	36.3	25.4	28.8	20.5
II	7	M	61.8	44.4	41.7	19.3	33.9	15.7
	8	F	58.0	47.0	33.4	20.0	26.4	16.0
III	9	F	61.3	46.5	40.6	21.3	32.3	16.2
	10	M	55.2	48.1	27.2	20.8	21.6	16.2
	11	M	52.3	41.7	28.7	16.5	21.6	12.7
IV	12	M	41.5	28.4	17.2	10.7	14.1	9.0
	13	M	36.2	28.5	13.1	9.9	11.3	8.5
	14	M	27.4	19.5	10.6	7.2	9.1	6.7

I. Blond and brown-haired Whites; II. Black-haired Whites; III. Mongoloids; IV. American Negroes.

A comparison of the changes in reflectance with different filters may help to distinguish increased melain from erythema. Light transmitted through the blue and green filters is absorbed by hemoglobin as well as by melanin, but light transmitted through the red filter is reflected from blood pigments. The difference between the readings with blue and red filters appears to increase especially in the 6 light-haired White individuals but does not occur among the three Negroes, in whom either there is little tendency to erythema or it is masked by melanin. Only one individual, the darkest Negro, shows a greater relative drop in reflectance with the red than with the blue filter.

DISCUSSION

Normal color of living human skin comes from various sources: melanin, melanoid, carotene, oxyhemoglobin, and reduced hemoglobin. The melanin in the epidermis and the hemoglobin in the superficial blood vessels of the dermis play the most important role in determining the skin color (Brunsting and Sheard, 1929). Edwards and Duntley (1939a) have analyzed the human skin color with a Hardy spectrophotometer and confirm the histological finding that the variation in quantity of melanin in epidermis is responsible for the difference in racial coloration. They also state that when there is much melanin it prevents the superficial blood from attaining visibility.

TABLE 2

Decrease in reflectance of the skin after ultraviolet irradiation.

			ABSOLUTE DECREASE IN REFLECTANCE			DECREASE AS PER CENT OF INITIAL REFLECTANCE		
GROUP	SUBJECT	SEX	RED	GREEN	BLUE	RED	GREEN	BLUE
I	1	M	6.0	17.9	14.4	8.8	38.2	36.0
	2	M	2.1	10.9	10.6	3.2	24.1	27.6
	3	M	2.2	9.1	7.8	3.4	21.1	20.2
	4	M	4.2	15.2	12.2	6.8	36.8	36.1
	5	M	3.8	7.5	7.7	6.2	18.7	23.3
	6	M	5.7	10.9	8.3	9.5	30.0	28.8
II	7	M	17.4	22.4	18.2	28.2	53.7	53.7
	8	F	11.0	13.4	10.4	19.0	40.1	39.4
III	9	F	14.8	19.3	16.1	24.1	47.5	49.8
	10	M	7.1	6.4	5.4	12.9	23.5	25.0
	11	M	10.6	12.2	8.9	20.3	42.5	41.2
IV	12	M	13.1	6.5	5.1	31.6	37.8	36.2
	13	M	7.7	3.2	2.8	21.3	24.4	24.8
	14	M	7.9	3.4	2.4	28.8	32.1	26.4

I. Blond and brown-haired Whites; II. Black-haired Whites; III. Mongoloids; IV. American Negroes.

In sun-tanning, two main factors affect the darkening of the skin: first, increased hemoglobin (erythema); second, increased melanin. We found that in the first two days, the initial erythema stage, there is a decrease in reflectance with the green and the blue filters, but no change with the red. Because hemoglobin in the skin does not absorb the light rays transmitted by the red filter, an increase in hemoglobin in the erythemal stage does not cause any appreciable change in reading with the red filter. Readings with the red filter can therefore be considered as an index of melanin. The rays which come through the blue and green filters can be absorbed by hemoglobin as well as melanin, so the readings with either of these two filters represent the summation of the absorption by melanin and of the effects of changed vascularization. The light-haired Whites and the Negroes show relatively little increase in absorption with the red filter; so there is apparently little increase in the relative melanin content of the skin after ultraviolet irradiation. In the two black-haired Whites and the three Mongoloids the relative increase in melanin appears to be much greater. The green and the blue filters show considerable change in absorption in all 8 Whites and all three Mongoloids, suggesting that there is a large degree of erythema affecting the darkening of skin color at the end of the course of ultraviolet irradiation. This is especially true in the light-haired Whites. The skin color of the Negroes changes very little in absorption with either green or blue filters, so the erythema effect is either weak or obscured by the layer of heavy melanin in the epidermis.

On the basis of histological findings, Keller (1928) and Miescher (1930) say that in the first stage of sun-tan, melanin migrates from the basal layers of the epidermis upwards, and that there is no actual increase in amount. Jansen (1953), using reflection spectrophotometry, shows that there is an upward migration of melanin on the eighth day after a single dose of ultraviolet rays. The darkening of the skin caused by melanin concentration is not strictly a surface phenomenon. Reflectance is not a function of, nor closely comparable with transmission of light. Therefore, it cannot *a priori* be assumed that absorption is proportional to the logarithm of the density of melanin. In the absence of empirical data bearing on the question we hesitate to estimate the extent of change in melanin from the degree of change in reflectance.

Edwards and Duntley (1939b) gave a single large exposure to sunlight to a single individual and found that the maximal increase in melanin was not reached until the nineteenth day. In our experiment with repeated small exposures, the maximal darkening was usually achieved between the seventh and the ninth day.

Hausser and Vahle (according to Kuppenheim and Heer, 1951-52) found that it required about 10 times as much ultraviolet energy to produce the same erythema in a Negro subject than it did in Whites. We found that, on the average, it takes 2.2 times as much irradiation in Negroes and 1.3 times as much in Mongoloids to produce the same minimal perceptible erythema as in Whites.

CONCLUSIONS

Using a red filter, so that the effect of erythema is minimal and that of tanning maximal, the amount of light reflected following repeated ultraviolet irradiation may be reduced by from 3 to 31%. Even in two individuals with similar initial reflectance or of similar racial background, the change in one may be several times as great as that in the other. The relative decrease in reflectance with a red filter is smallest in blond and brown-haired Whites, and greatest in Negroes. However, the range in Monogoloids and black-haired Whites overlaps that of the Negroes. Use of green and blue filters records the effects of erythema as well as melanin. Except for the darkest Negro individual all subjects showed relatively more change in reflectance measured with the blue than with the red filter. The results of this experiment confirm that there are wide individual differences in suntanning. The limited observations do not reveal any specifically racial characteristic in this respect, however.

ACKNOWLEDGMENT

We cordially appreciate the zeal of our volunteers in participating in this experiment. Gratitude is also extended to Dr. J. J. Groen, of the Department of Industrial Medicine, Wayne State University, for answering some of the authors' questions about spectral physics.

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BONE STRENGTH—ITS RELATIONSHIP TO X-RAY-DETERMINED ASH CONTENT[1]

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THE measurement of bone strength has long been a fascinating research area for biologists and anatomists. Although weighing only one-third as much, bone has the tensile strength of cast iron under static loading; and its elasticity permits it to absorb impact loads several times higher. There have been relatively few studies on the strength of human bone, although the number of investigations has increased in recent years. Evans and Lebow (1951, 1952) measured the tensile strength of compact human bone and found significant strength variations among samples from different parts of the same bone. Carothers, Smith, and Calabrisi (1949) tested the effects of drying and embalming upon the strength of compact bone. In the investigation which is the subject of the present report we have produced transverse fractures through bending of the femur by applying force to the middle of the shaft perpendicular to its long axis, and have related breaking stress obtained by this method to bone ash content.

In an earlier report (Vose, 1958b) a new analytical procedure, employing a low intensity polychromatic x-ray beam, was applied specifically to the determination of the organic and inorganic contents of dried, ground bone. With 1.0- to 1.5-gram samples the concentration of the two fractions is related exponentially to the per cent x-ray transmittance, thus providing an expedient and accurate means for measuring mineral content without the need for ashing the bone. This method has been used to determine the ash content of the bones used in this study.

The factors which affect the strength of bone still are not clearly understood, but any changes in the quality or quantity of either the organic or inorganic fractions may result in changes in bone strength. Wier, Bell, and Chambers (1949), working with rats that had been

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maintained on rachitogenic diets, found a positive correlation between breaking stress and per cent ash content in the femur. Hence bone strength in animals has been found to be measurably related to its composition.

METHODS

Twenty-five human femurs were obtained for breaking stress and x-ray-determined composition measurements. All but two of the femurs had been removed from embalmed white and Negro cadavers of both sexes between the ages of 30 and 70 years. The remaining pair was taken from a twelve-year old white female. All the bones were cleaned and the periosteum removed. Twenty-one femurs were dried at room temperature for 30 days before the breaking tests, and four were soaked in Ringer's Solution and tested wet.

Determination of Bone Breaking Stress

The femur was selected for strength and ash analysis because it is the largest bone in the body, and because its approximate elliptical shape make the determination of its moment of inertia relatively simple. Admittedly, the variations from the proportions of a true ellipse among individual bones will alter the apparent breaking stress to a slight degree.

To obtain the breaking load, each femur was supported horizontally and force was applied downward upon the center of the posterior surface of the shaft by use of a specially constructed 8000-pound capacity hydraulic testing device. The principal components of the device were (1) a rack constructed from $\frac{1}{4}$-inch steel which was approximately 50 inches high, 30 inches wide, and 10 inches deep. Suspended within the rack was (2) a hardwood cabinet with a hinged Plexiglas window through which the bone could be observed while force was being applied, and (3) mounted atop the rack was a Black Hawk hydraulic Porto-Power pump which applied force downward through a steel plunger of adjustable length. Two pressure gauges were used interchangeably in the hydraulic system. A 1000-pound capacity gauge was used during the greatest number of tests, but several of the bones required a 2000-pound gauge in order to obtain the breaking load. Force was applied manually at the rate of about 500 pounds a minute. The strain gauge was an integral part of the hydraulic plunger which indicated the maximum amount of bending at the instant of fracture. The reading was retained on the gauge until the scale was reset by the operator.

Each bone was supported within the cabinet and force was applied tangent to the axis of natural curvature which tended to stabilize the bone upon its end supports. The supports were constructed from oak blocks which were hollowed out to approximate closely the dimensions of the distal and proximal anterior surfaces. The supports were adjusted to the effective span for each femur as measured from a point just below the lesser trochanter to a point immediately above the adductor tubercle. In this manner the strength of the compact bone of only the femur shaft was obtained, and the structural weakness of the extremities was avoided. Next, the hydraulic plunger was adjusted to the surface of the bone and the attached strain gauge adjusted to zero. Force was then applied at this loading site until fracture by bending occurred, and the breaking load in pounds (subsequently converted into kilograms) and the deformation in millimeters were read directly from the corresponding gauges.

The breaking load, which may vary between 150 and 600 kilograms in embalmed normal femurs, is a measure of the strength of the bone as a whole witout regard for bone size. In order to convert the breaking load into breaking stress or force per unit area, certain dimensions of the bone had to be determined as indicated in Figure 1(A). The strength of the bony material was calculated by a method similar to that used by Wier, Bell, and Chambers (1949) according to the formula

$$S = \frac{W\,l\,D}{8\left[\frac{\pi}{64}\,(BD^3 - bd^3)\right]}$$

Where

S = Breaking stress by bending
W = Breaking load
l = Span of femur shaft
B = External width at stress point
b = Internal width at stress point
D = External depth at stress point
d = Internal depth at stress point.

X-ray Determination of Ash Content

The total femur length between the proximal border of the greater trochanter and the distal border of the medial condyle was measured and divided into three sections of equal length. As shown in Figure 1(B) a two-centimeter section was removed at the midpoint of each one-third. All of the soft material within the medullary cavity was removed. The remaining compact bone was broken into small pieces and then pulverized in a Waring Blender, and each sample obtained in this manner was dried to constant weight in an Ohaus Moisture Determination Balance.

In making the x-ray measurements, a General Electric KX-8 Generating Unit was used with an HRT-2 rotating tungsten anode tube.

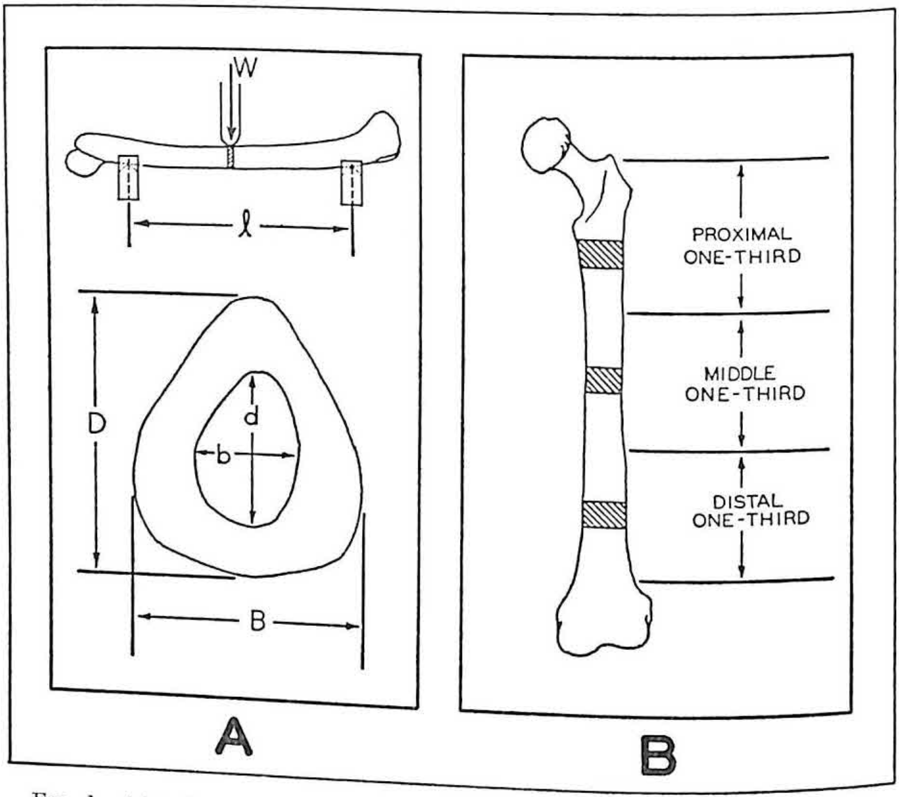

Fig. 1. (A) Cross-sectional and Longitudinal Views of Supported Femur Indicating Dimensions Used in the Breaking Stress Computations.

(B) Sites from which Samples were Removed (cross hatched) from Each One-third of the Femur Shaft.

As in previous work (Vose, 1958b) all filtration was removed from the tube housing so that the relatively soft beam produced at 40 kvp was utilized. A Model 287 Victoreen Minometer with a 0 to 0.1-r chamber was used to measure the transmitted x-radiation. Samples of 1.5 grams of dried, pulverized bone were placed in a cylindrical sample chamber which compressed each sample of bone material into a small volume. The chamber was designed to fit into a steel cylinder mounted above the x-ray tube so that the beam was conducted vertically through the cylinder and sample chamber, and into the ionization chamber. The per cent x-ray transmittance for each sample was obtained in two steps, first by measuring the transmittance through the 1.5-gram sample, second by measuring the transmittance through the empty sample chamber. The per cent transmittance was obtained by the usual formula, $100\,I/I_0$. The exponential relationship between the x-ray transmittance and the per cent ash content for 1.0- and 1.5-gram samples is shown in Figure 2.

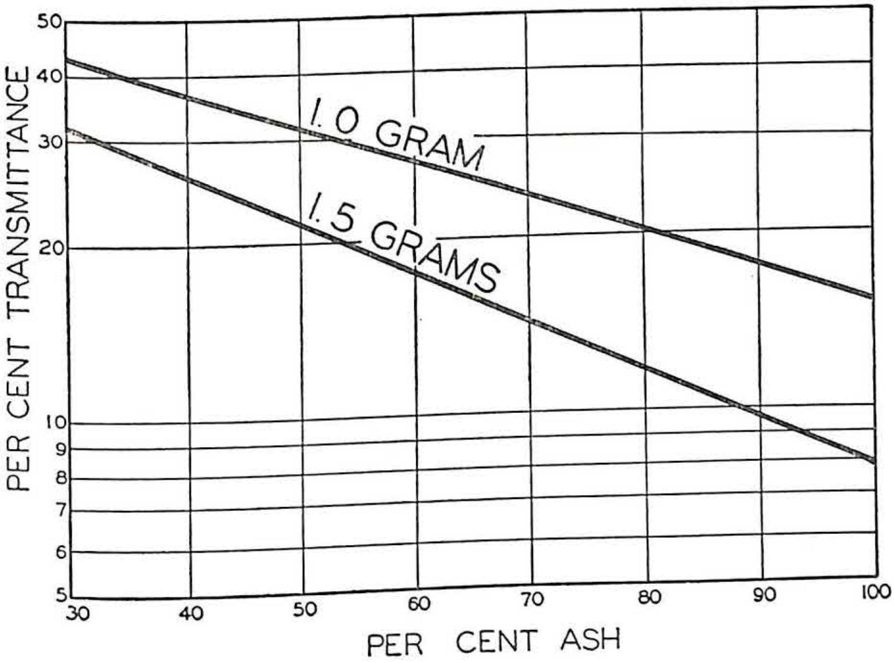

FIG. 2. RELATIONSHIP BETWEEN PER CENT ASH AND PER CENT TRANSMITTANCE IN 1.0- AND 1.5-GRAM SAMPLES OF DRIED, GROUND BONE TISSUE.

RESULTS

The results of plotting x-ray-determined ash content vs breaking stress of the femur revealed that the two factors were related in an exponential manner. A plot of breaking stress vs ash content is shown in Figure 3. The figure shows that there is an obvious relationship between the two variates, and a statistical treatment to determine what kind of mathematical curve best describes the observed relationship has been made.

First, a straight line was fitted by least squares, and a product moment correlation coefficient of .94 was obtained. This coefficient is significant well beyond the .001 level. It was found that a second degree polynomial yielded somewhat smaller errors of prediction, but a rational defense of a parabolic relationship with this type of data seemed untenable, so an exponential curve was tried. Because of the extreme difficulties in fitting exponentials by the method of least squares, the curve was determined in a different manner. Three points were chosen on the plot through which it seemed likely that the curve should pass, and these points were employed to obtain three equations of the form $y = ae^{bx} + c$, where x, the independent variable, is per cent ash content. The three constants were then determined and the equation obtained was $y = 1.304e^{.202x} + 2.210$. This curve is the one reproduced in Figure 3.

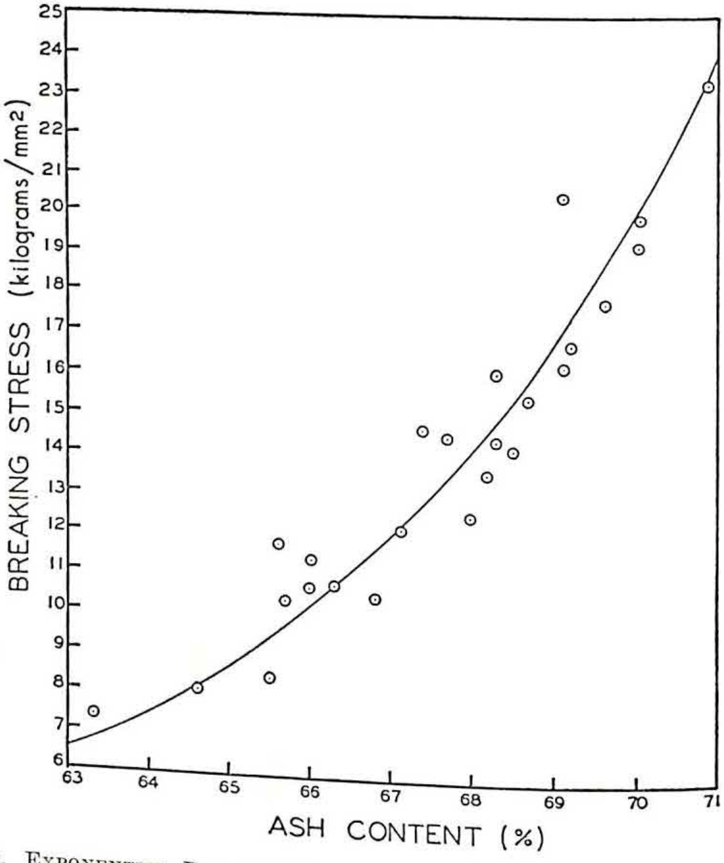

FIG. 3. EXPONENTIAL RELATIONSHIP BETWEEN BREAKING STRESS IN KG/MM² AND CORTICAL ASH CONTENT OF THE FEMUR.

The exponential curve yielded smaller errors of prediction than the rectilinear curve, but the prediction was not significantly better. It might be argued that the principle of parsimony would favor the acceptance of a simpler, or rectilinear form of relationship, but one other important fact must be taken into consideration. Employing the line of best fit in this data, it would be predicted that a femur with a cortical ash content of 60.8% would have a breaking stress of zero. Since ash contents as low as 54% of the dry skeletal weight have been reported (Trotter and Peterson, 1955) the straight line determined from our data obviously cannot represent the true relationship between breaking stress and ash content. This factor, plus the fact that the exponential curve did fit the data somewhat better, seemed to warrant the postulation of an exponential form of relationship between the two variables.

No clearly defined correlation between strain per unit length and breaking stress was found. It is possible that a correlation might have been found had all of the bones been of the same degree of dryness.

DISCUSSION

In living bone, fracture may be caused by any of 5 types of straining —axial compression, axial tension, bending, shearing, and twisting. All of the foregoing loading conditions may result from either static or impact forces. Admittedly, bone fracture is more likely to occur through impact than under static loading conditions, but the latter can be measured with much higher precision. In addition, there is without question a relationship between impact and static yielding loads.

There are a number of inherent factors which complicate the determination of breaking stress by bending in bone. These are (1) both tensile and compressive forces are involved which are greatest on the convex (lower) and concave (upper) aspects respectively and decrease inwardly toward a neutral axis; and (2) the cross sectional area and shape varies along the shaft, and the walls are not of uniform thickness. Nevertheless, the breaking stresses by bending are related in a complex manner to the breaking stress caused by loading conditions which more commonly result in bone fracture. There are several factors which might affect the relationship between bone strength and ash content. Changes in the quality and quantity of bone mineral or collagen might result from such natural factors as age, sex, and race; and from such unnatural factors as the degree of dryness of the bone, and whether the bone is embalmed or unembalmed.

Evans and Lebow (1951) reported no consistent relationship between the age of the individual and bone strength, although some of the older literature is not in agreement with this (Rauber, 1876).

Differences in breaking stress accountable to sex are undoubtedly slight, although the breaking load is roughly related to bone size. Evans (1957) reported that the average breaking stress of samples from male femurs was only about 5% greater than similar samples from female femurs.

Although there is no known work in which the breaking stress of bones from Negroes and whites have been compared, Baker and Schraer (1958) reported slight differences in x-ray absorption coefficients among bones of equal weight from Negroes and whites.

Strength differences accountable to drying or embalming might cause fairly large errors. Evans and Lebow (1951) have reported that drying of bone samples from femurs of human cadavers increased their average tensile strength by about 30%, but reduced their percentage elongation under tension. Dempster and Liddicoat (1952) reported breaking stresses of fresh and dry bone samples which were in good agreement with the earlier findings of Evans and Lebow. In the study upon which this report was based, the right femurs of two cadavers were tested after being soaked in Ringer's Solution for three weeks, and the left femurs were tested after drying at 95° F. for three weeks. The wet bones were found to have breaking stresses by bending about 3% greater than those of the dry bones. This evidence suggests that drying at room temperature for reasonable lengths of time will not significantly affect breaking stress although higher drying temperatures undoubtedly will affect it. It was found that the cortex of three femurs dried at room temperature for 6 months still contained about 10 or 11% water. Since none of the bones in the study had been permitted to remain at room temperature for more than three weeks, the error accountable to the state of dryness appeared to be within 3%.

The influence of embalming upon the strength of compact bone was investigated by Calabrisi and Smith (1951) who used machined samples taken from various parts of femurs from embalmed dissecting room cadavers. Their measurements showed that embalming reduced the compressive strength of compact bone by about 13%.

It is obvious that the effect of certain postmortem alterations will affect bone strength in an abnormal manner. However, there is undoubtedly a relationship between the breaking stresses of dried, embalmed bones and their original breaking stresses within the host.

It would be extremely useful if bone strength could be predicted in the living human being. However, quantitative *in vivo* determinations of bone mineral concentration are difficult because of differential x-ray scattering by the bone matrix and the overlying soft tissue. In the case of bones having only thin coverings of soft tissue, such as the distal extremity of the femur, a method for direct quantitative measurement of the mineral content has been described by this laboratory by use of a collimated x-ray beam and a sensitive detection system (Vose, 1958a, 1959). Deeper coverings of soft tissue greately complicate *in vivo* measurements of bone mineral concentration, and further work must be done before the scattering effect by soft tissue can be completely corrected. If this can be accomplished with sufficient accuracy, it will be possible to estimate bone strength *in vivo* since it is related in a known manner to the ratio of its organic and inorganic components.

SUMMARY

The breaking loads through bending for 25 dried, embalmed human femurs have been measured by use of an 8000-pound hydraulic testing machine. The breaking stresses were computed through application of dimensional factors of the individual bones. Next, a new analytical procedure employing a low intensity polychromatic x-ray beam was used to determine the ash content of samples removed from three sites in each femur, and the breaking stresses and average ash contents were correlated and found to be related by an exponential function. No clearly defined correlation between strain per unit length and breaking stress by bending was found.

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BIAS IN POLIOMYELITIS RATES BASED ON EXPOSED-TO-RISK FORMULAS

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INTRODUCTION

THE customary method of computing an annual rate of an event according to exposed-to-risk formulas is:

$$\frac{\text{Number of events in population}}{\text{Average size of population during year}} \tag{1}$$

It has been pointed out by Bradford Hill (1950) that in situations in which incidence rates of the event in question show marked seasonality and the population at risk also undergoes substantial reduction or increases in size during the observation period, this method of computation can give misleading results. This type of situation was recently encountered in the field of epidemiology. The problem involved measuring the effectiveness of the poliomyelitis vaccine based on the poliomyelitis incidence rates of vaccinated persons and unvaccinated persons in the United States during 1957. During that year, the population was participating heavily in the poliomyelitis vaccination program, and the marked seasonality of the disease is well known.

The problem can be illustrated by a somewhat extreme hypothetical example. Suppose that in a certain population deaths from a certain cause occur at the constant rate of 5 per million during the first half of a calendar year and at the constant rate of 10 per million during the second half, while the size of the population is 2 million during the first half and 1 million during the second. A simple calculation shows that the average annual death rate by formula (1) is $6\frac{2}{3}$ per million. Now consider another population in which the respective death rates are 4 and 8 per million and the corresponding populations are 1 million and 2 million. Calculation again gives an average rate of $6\frac{2}{3}$ per million, even though the death rate in each half of the year was lower than in the first example.

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[*] Thanks are due to Dr. Carl C. Dauer of the National Office of Vital Statistics for encouragement to study this problem and to Dr. Lauri D. Thrupp, Miss Helen Forester and Dr. Jacob A. Brody of the Poliomyelitis Surveillance Unit of the Communicable Disease Center U. S. PHS, Atlanta, Georgia, for practical assistance in getting tabulations of the poliomyelitis cases.

The bias resulting from the combination of seasonality of incidence rates with population fluctuations can be eliminated by computing a cohort rate—that is, the proportion of occurrence of the given event which would result in a closed cohort subject in every part of the year to the same risk of occurrence of the given event as the actual population under study. This is the same procedure that is used when a life table is constructed from the age-specific death rates observed in a population during a specified period.

In practice, such a cohort rate cannot be computed exactly because the population count is available only for a few dates during the year and the number of occurrences of the given event is available only for certain limited subdivisions of the year. In the subsequent section of this report, a computational procedure is outlined which provides upper and lower bounds on the cohort rate. In next to the last section, this procedure is applied to poliomyelitis data available for the United States in 1957. In the final section, the poliomyelitis rates and effectiveness of the vaccine are computed by formula (1) and compared with those computed by the suggested procedure.

PROCEDURE FOR COMPUTING RATES

The computational procedure has most relevance to situations for which estimates of the number of events are available for subdivisions (not necessarily of equal size) of the year, and estimates of the population at risk are available for each subdivisional period. The procedure involves (1) computing the rate of each subdivision of the year, and (2) adding the subdivisional rates to obtain the annual rate.[1]

The formula for computing the subdivisional rate is:

$$\frac{\text{Number of events in the population during the subdivision of the year}}{\text{Size of population at a date during the subdivision of the year}} \tag{2}$$

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[1] As shown in the Appendix, this applies when the annual rate is not large.

The mathematical theory derived in the appendix of this report states that the denominator of the rate is equal to the population size at some date during the subdivisional period. If an estimate of the population size at a single date during the period is available, it would be used for the denominator.

In the event that the minimum and maximum sizes of the population during the period are known, upper and lower bounds of the rate for that period may be derived. Substitution of the minimum population size in the denominator would give the upper bound and substitution of the maximum population size would give the lower bound of the subdivisional rate. The point estimate of the subdivisional rate would be obtained by use of a value of the population size in the denominator that is at a point (say midway) between the minimum and maximum population sizes.

The lower bound of the annual rate is equal to the sum of the lower bound rates for the subdivisional periods and the upper bound is equal to the sum of the upper bound rates for those subdivisional periods. The difference between the upper and lower bounds of a rate for a period of fixed length can not be increased and may be decreased by further subdivision. Thus, the reliability of the annual rate is in general improved by increasing the number of periods into which the year is subdivided. The detail of data available will usually place a limit on the number of subdivisions since information pertaining to the number of events occurring during the period and estimates of the population size would be needed for each period. On the other hand, if further subdivision of the year does not appreciably improve the accuracy of the annual rate, it would be inefficient to do so. From this viewpoint, the nature and extent of subdivision of the year becomes a matter for empirical investigation.

POLIOMYELITIS RATES AND VACCINE EFFECTIVENESS

The Salk poliomyelitis vaccine is administered in three doses. In 1957 medical authorities recommended that the second and third doses should be spaced about one month and seven months, respectively, after the first dose. In view of the short spacing interval between the first and second doses, poliomyelitis rates in this report are computed for the combined classes representing persons who received one or two inoculations rather than for each class separately.

The extent of participation of the population under 20 years of age in the poliomyelitis inoculation program at selected dates during 1957 is shown in table 1. The estimates are based on data collected in two national household surveys—one conducted during the third week of August (Sirken and Brenner, 1959); the other conducted during the third week of November. The surveys were sponsored by the Public Health Service and conducted by the Bureau of the Census as supplements to the Current Population Survey. The Current Population Survey uses a probability sample design covering the non-institutionalized civilian population of the continental United States (Current Population Reports, 1958).

TABLE 1

Estimates of the poliomyelitis inoculation status of the U. S. population under 20 years at selected dates during 1957

DATE	POPULATION (IN 1000'S) BY NUMBER OF INOCULATIONS RECEIVED		
	NONE	1 OR 2	3 OR MORE
January 1, 1957	30,648	21,796	9,694
February 15, 1957	28,045	21,330	12,950
April 1, 1957	25,442	20,864	16,206
May 15, 1957	22,506	18,815	21,462
July 1, 1957	19,570	16,766	26,718
August 15, 1957	18,096	16,801	28,419
October 1, 1957	18,266	15,604	29,736
November 15, 1957	18,436	14,405	31,054
January 1, 1958	18,356	14,018	31,810

About the same number of persons were vaccinated and unvaccinated at the beginning of the calendar year but by the end of the year, the vaccinated population was about 2½ times larger than the unvaccinated population. The increase in the vaccinated population was due entirely to the increase in the number of triply vaccinated persons since the number of persons with one or two inoculations declined steadily throughout the year.

The surveys provided estimates of the population's participation in the inoculation program as of the middle of the third and fourth quarters of 1957. Estimates of the inoculation status of the population at the beginning of the first, second and third quarters of 1957 were derived by retrojecting the experience of the cohort interviewed in August on the basis of the reported date of the last inoculation and assuming prescribed waiting periods between earlier inoculations. Estimates of the inoculation status of the population at midpoint of the first and second quarter were obtained by linear interpolation. Estimates of inoculation participation at the beginning of the fourth quarter were obtained by linear interpolation between the two survey results. Estimates for the end of the fourth quarter were derived by projecting the intentions of the cohort interviewed in November. Finally, the retrojected estimates based on the August cohort and the projected estimates based on the November cohort were appropriately adjusted to reflect changes in the population size during 1957.

TABLE 2

Poliomyelitis cases reported by quarter of onset and inoculation status: U. S. population under 20 years, 1957 *

	NUMBER OF INOCULATIONS RECEIVED		
QUARTER OF ONSET	NONE	1 OR 2	3 OR MORE
Paralytic and Non-Paralytic Cases			
Total	1,823	1,044	745
1st quarter	190	70	29
2nd quarter	380	183	134
3rd quarter	1,021	703	529
4th quarter	232	88	53
Paralytic Cases			
Total	1,120	382	192
1st quarter	133	34	14
2nd quarter	224	64	22
3rd quarter	571	229	117
4th quarter	192	55	39

* Data in this table were supplied by the Poliomyelitis Surveillance Unit; Communicable Disease Center U. S. PHS; Atlanta, Georgia.

The survey estimates of the inoculation status of the population for August 15 and November 15 appear to be highly reliable. Relative sampling errors of the population estimates are less than 2% (1 standard deviation) and according to check interviews in a resurvey of a subsample of households, response bias was not a significant factor. The retrojected estimates of the population with one or two inoculations may be slightly too large because the average spacing between successive inoculations appears to have been somewhat longer than that assumed. For example, January and February 1956 were retrojected dates of first and second inoculations of persons who reported getting their third inoculation during August 1957 assuming that the second and third inoculations were administered six and seven months, respectively, before the third inoculation. In actuality, however, these persons were more likely to have obtained their first and second inoculations prior to than after these dates. The projected values of the uninoculated population may be slightly overstated because usually these persons tended not to get inoculated as soon as the expected dates which they reported in the survey.

TABLE 3

Bounds on the annual poliomyelitis rate according to the manner of subdividing the year by inoculation status of the population: United States population under 20 years, 1957

SUBDIVISIONS USED TO COMPUTE RATE	ANNUAL RATE (PER 1,000,000) LOWER BOUND	UPPER BOUND	RANGE
Paralytic and Nonparalytic Cases			
No inoculations			
Without subdivision	59.5	100.7	41.2
2 semi-annual periods	82.6	98.4	15.8
4 quarterly periods	85.9	96.0	9.1
One or two inoculations			
Without subdivision	47.9	74.5	26.6
2 semi-annual periods	58.8	71.5	12.7
4 quarterly periods	59.6	65.6	6.0
3 or more inoculations			
Without subdivision	23.4	76.8	53.4
2 semi-annual periods	24.4	38.6	14.2
4 quarterly periods	26.3	32.8	6.3
Paralytic Cases			
No inoculations			
Without subdivision	36.5	61.9	25.4
2 semi-annual periods	50.6	60.4	9.8
4 quarterly periods	52.7	58.7	6.0
One or two inoculations			
Without subdivision	17.5	27.3	9.8
2 semi-annual periods	21.4	26.1	4.7
4 quarterly periods	21.8	24.0	2.2
3 or more inoculations			
Without subdivision	6.0	19.8	13.8
2 semi-annual periods	6.2	9.6	3.4
4 quarterly periods	6.8	8.5	1.7

The number of reported cases of poliomyelitis under 20 years of age in the United States during 1957 by quarter of onset of the disease and number of inoculations received prior to the onset of the disease are shown in table 2. It will be noted that poliomyelitis is a seasonal disease with most of the cases occurring during the third quarter of the year. The data are shown separately for confirmed paralytic polio cases.

Applying the procedure for computing rates outlined in the previous section to the data contained in tables 1 and 2, upper and lower bounds of the poliomyelitis rates were derived separately for persons with no inoculations, one or more inoculations, and three or more inoculations. The upper and lower bounds were computed under three sets of conditions with regard to subdividing the year: (a) without subdivisions, (b) two semi-annual periods, and (c) four quarterly periods. The results pertaining to rates for combined paralytic and nonparalytic cases and for paralytic cases only are presented in table 3. The figures show substantial reductions in the range between the upper and lower bound with increases in number of subdivisions used to compute the rate.

TABLE 4

Bounds on the estimates of the vaccine effectiveness by number of inoculations received: United States population under 20 years, 1957

INOCULATION STATUS	PERCENT EFFECTIVENESS OF VACCINE		
	LOWER BOUND	UPPER BOUND	RANGE
Paralytic and Nonparalytic Cases			
One or two inoculations	23.6	38.0	14.4
Three or more inoculations	61.8	72.6	10.8
Paralytic Cases			
One or two inoculations	54.4	62.9	8.5
Three or more inoculations	83.9	88.4	4.5

The effectiveness of the poliomyelitis vaccine is measured by percentage reduction in the poliomyelitis rate for persons with inoculations as compared to the rate for persons without inoculations. Assuming that the year is subdivided into four quarters for purposes of computing the rate, table 4 contains upper and lower bounds on estimates of the vaccine's effectiveness. Greater precision in the estimate of vaccine effectiveness would have been obtained if finer subdivisions of the year had been used in calculating the poliomyelitis rates.

COMPARISON OF THE METHODS FOR COMPUTING RATES

In the second section of this report, it was suggested that a point estimate of the rate for each subdivision would be obtained by setting the denominator of formula (2) equal to the average of the maximum and minimum population sizes during the quarter. On this basis annual rates were computed based on 4 quarterly subdivisions of the year. Annual rates were also computed by use of formula (1). In this latter instance, the denominator was set equal to the average of the 4 population bases that were used to obtain point estimates of quarterly rates. Estimates of the rates computed by both methods are shown in table 5.

TABLE 5

Comparison of the estimates of poliomyelitis rates as derived according to two computational methods: United States population under 20 years, 1957

TYPES OF POLIOMYELITIS	ANNUAL RATE (PER 1,000,000) COMPUTED BY USUAL METHODS	ANNUAL RATE (PER 1,000,000) COMPUTED BY RECOMMENDED METHODS
Paralytic and Nonparalytic		
No inoculations	83.1	90.5
1 or two inoculations	58.7	62.4
3 or more inoculations	31.9	28.9
Paralytic		
No inoculations	51.1	55.5
1 or two inoculations	21.5	22.8
3 or more inoculations	8.2	7.5

The rates for persons with 3 or more inoculations are higher when computed by formula (1), and the rates for persons with no inoculations and one or two inoculations are higher when computed by formula (2). The explanation for this is that in general formula (2) relates the date of onset of poliomyelitis cases more particularly to the population size exposed as of that date. In this example the exposure of the triply vaccinated group was more heavily concentrated in the quarters of the year during which the risks of getting poliomyelitis were greatest. It is noteworthy that the estimates of poliomyelitis rates for persons with no inoculations and for persons with one or more based on formula (1) are not contained within the bounds shown in table 3, and the estimates of the rates for persons with 3 or more inoculations are barely within the bounds.

The percent effectiveness of the vaccine based on both sets of estimates is shown below:

	USUAL METHODS	RECOMMENDED METHODS
Paralytic and Nonparalytic Cases		
1 or two inoculations	29.4%	31.0%
3 or more inoculations	61.6	68.0
Paralytic Cases		
1 or two inoculations	57.9	59.9
3 or more inoculations	83.9	86.4

Compared to rates computed by recommended methods, those computed by formula (1) tend to understate the vaccine's effectiveness. The estimates of vaccine effectiveness of three or more inoculations based on formula (1) are not contained within the bounds of the vaccine's effectiveness shown in table 4.

The possible misinterpretations resulting from the rates of the vaccine's effectiveness based on formula (1) are not necessarily confined to the present but may present problems in the future as well. Assuming that estimates of the vaccine's effectiveness will be computed in future years, they may raise questions in 1958 or 1959 or later as to why vaccine appeared to be less effective in 1957.

SUMMARY

Had poliomyelitis incidence rates of vaccinated persons and unvaccinated persons in the United States in 1957 been computed in the customary manner according to exposed-to-risk formulas, they would have been biased. The rates for triply vaccinated persons would have been overstated and those for unvaccinated persons and for persons who had been administered one or two inoculations would have been understated. The effectiveness of the vaccine, particularly when three doses had been administered, computed on the basis of these erroneous rates, would have been understated.

In general, the customary manner of computing rates according to exposed to risk formulas can give misleading results in situations in which incidence rates of the event show marked seasonality and the population at risk also undergoes substantial reduction or increase in size. This was the situation with regard to poliomyelitis incidence rates of vaccinated and unvaccinated persons in the United States in 1957. During that year, the population was participating heavily in the poliomyelitis vaccination program, and the marked seasonality of the disease during the summer and autumn months was evident.

Since population counts of the vaccinated and unvaccinated population based on national household surveys were available only for a few dates during 1957, it was not feasible to compute unbiased cohort rates. In this paper, a method was developed for computing upper and lower bounds of the cohort rates which requires no more information than was available. The method was applied to poliomyelitis data for the population under 20 years of age and the results compared to those obtained by application of the conventional exposed-to-risk formulas. In view of the misleading results obtained by the exposed-to-risk formulas in this paper, the suggested procedure is being applied to poliomyelitis data by age, sex and geographic division available for the United States for 1957 to obtain measurements of the vaccine effectiveness.

The substantive findings regarding the vaccine's effectiveness are of interest in their own behalf. According to the results presented in this paper, three or more inoculations of Salk vaccine to the population under 20 years were between 83 and 89% effective against paralytic poliomyelitis. This result compares favorably with the measurement based on the 1954 field trial of poliomyelitis vaccine (Francis, *et al.*, 1957) which indicated that 3 inoculations of the vaccine to children 7, 8 and 9 years of age were about 80% effective against paralytic poliomyelitis.

APPENDIX

Let μ_t $(0 \leqq t \leqq 1)$ denote the force of mortality at time t from the beginning of the year. Then the cohort annual rate of mortality is

$$q = 1 - e^{-\int_0^1 \mu_t \, dt}.$$

Thus, we can compute q if we can determine

$$\int_0^1 \mu_t \, dt = -\log_e(1-q).$$

Let $N(t)$ denote the number in the population under study at time t, and let D denote the number of deaths during the year. Then

$$D = \int_0^1 N(t)\mu_t \, dt = \sum_{i=1}^{m} \int_{t_{i-1}}^{t_i} N(t)\mu_t \, dt,$$

where

$$t_0 = 0, \qquad t_m = 1, \qquad t_i > t_{i-1},$$

If $N(t)$ is continuous and μ_t is nonnegative in each interval $t_{i-1} < t < t_i$,

$$D = \sum_{i=1}^{m} N(\xi_i) \int_{t_{i-1}}^{t_i} \mu_t \, dt = \sum_{i=1}^{m} D_i,$$

where

$$t_{i-1} < \xi_i < t_i.$$

Thus, $\int_{t_{i-1}}^{t_i} \mu_t \, dt$ can be estimated from $\int_{t_{i-1}}^{t} \mu_t \, dt = \frac{Di}{N(\xi_i)}$,

and

$$-\log_e (1-q) = \sum_{i=1}^{m} \frac{Di}{N(\xi_i)}$$

or

$$q = 1 - e^{-\sum_{i=1}^{m} \frac{Di}{N(\xi_i)}}$$

Since the fraction $q' = \sum_{i=1}^{m} \frac{Di}{N(\xi_i)} < 1$, the function $e^{-q'}$ is a convergent series

$$e^{-q'} = 1 - q' + \frac{(q')^2}{2!} - \frac{(q')^3}{3!} + \cdots.$$

Therefore, q may be rewritten as

$$q = q' - \frac{(q')^2}{2!} + \frac{(q')^3}{3!} - \cdots.$$

In applications for which q' is a small fraction, it serves as a reliable estimate of q.

LITERATURE CITED

BUREAU OF THE CENSUS 1958 Concepts and methods used in the current employment and unemployment statistics. Current Population Reports, Series P 23, No. 5.

FRANCIS, THOMAS, JR., *et al.* 1957 Evaluation of the 1954 Field Trial of Poliomyelitis Vaccine. Edwards Brothers, Inc., Ann Arbor, Michigan, pp. 272-276.

HILL, BRADFORD 1950 Principles of Medical Statistics. 5th edition, The Lancet Limited, London, p. 182.

SIRKEN, MONROE, AND BERTHOLD BRENNER 1959 Poliomyelitis vaccination participation differentials: United States, August 1957. Unpublished manuscript.

BOOK REVIEWS

The scope of physical anthropology and its place in academic studies. By J. S. WIENER and D. F. ROBERTS. 66 pp. Publ. for *Soc. for Study of Human Biol.* by Wenner-Gren Foundation for Anthropological Research, New York, N. Y., 1958. (Privately printed, not for sale. Now out of print).

Some years ago there was a radio character whose tag-line was, "That ain't the way I heerd it, Johnny!" In similar vein modern physical anthropology "ain't the way I learned it!", if this volume is any criterion. On Nov. 6, 1957, the Ciba Foundation sponsored a Symposium by British biologists especially interested in physical anthropology. The Wenner-Gren Foundation published the papers—*not* the accompanying discussion—and the Society for the Study of Human Biology was born of the Symposium. So much for background.

This is a double-barrelled approach: first, the scope of physical anthropology, secondly, how it fits into an academic program. The first can be answered by a generalization that all's grist that comes to and from the biological mill, i. e., there's nothing about Man's form and function that is not apropos to physical anthropology. The second is answered rather obliquely by Young: "I believe that the objective study of man by biological techniques such as an anthropologist can use provides as firm a foundation as any other for a liberal education."

To speak in a purely American lingo, you can't get to first base as a physical anthropologist today unless you are well grounded in genetics, physiology, biochemistry and microbiology. The traditional Measurer of Man—*ens corporis*—has given way to the measurement of tissues, cells, excretions, and energy-exchange systems. The whole has surrendered to its parts—nay, its fractions—with a vengeance! You no longer measure what you see: you experiment to determine if what you see is meaningful or not. No longer the what (the observed form), but the how (the mechanics) and the why (emergent or selective causation).

In physical anthropology—which has a formal 1859-1959 history—we have gone through what Le Gros Clark calls the "romantic" phase, wherein empiricism ruled, the biometric phase, testing the variability of human populations, to a more proper and more detailed study of the biology of Man, past and present, with experimentation a dominant motif. In this third phase, the present, emphasis is upon human form and function in action, with respect to the external environment, as climatic factors, the work situation ("ergonomics," "occupational anthropology"), and so on.

Those of us who have been in the field over the years have seen the emergent trend, both in direction and in scope. I have long had the feeling that our field of human biology should be hand-maiden to the medical and dental specialities. This means that data derived from mass studies must be applicable to individual (clinical) instances. With this in mind I must confess that the volume under review worries me a bit. The direction is clear, but aim is not manifest. I am sure that this must have been realized and expressed via discussion at the Symposium, but the discussion is not given, and the papers, in their entirety are not integrative. In this sense Williams' "Biochemical Individuality" or Macy and Kelly's "Chemical Anthropology" did more thorough jobs. When all's said, however, the Symposium not only resulted in stimulating papers but in the establishment of a new Society. I am sure that synthesis will be only a matter of time.

WILTON MARION KROGMAN

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Methods for Research in Human Growth. By STANLEY M. GARN and ZVI SHAMIR. xi + 121 pp. Charles C. Thomas, Springfield, Ill. 1958. $4.75.

There is a first impression of surprise on connecting such a title with the size of this book. Can so vast a subject be dealt with in 121 pages? But in their introduction the authors make it clear that this is expressly a modest book and that each chapter could be expanded into a separate monograph. As they point out, there are many thousands of published investigations into this large field but very few describe methods—either general or more detailed. Is this book, then, a "do-it-yourself"? Not completely; what it does do, above all else, is to describe methods known to be acceptable and practical for the investigation of various aspects of physical growth, and to point out to prospective investigators the sort of thing they are getting into. This is a valuable service.

There are 21 chapters and the headings range from 'Skin' to 'Measurement of Strength and Muscle Function'; from 'Health Examination and Health Records' to 'Hair.' In reviewing the methods under each heading a very fair sample of bibliography is quoted, and the authors are never afraid to opinionate. This adds to the book's readability.

There are particularly good sections on introductory general considerations, radiation hazards, and dentition. There are some minor points which might cause differences of opinion and two are given as examples: First, in stressing the importance of reasonably exact timing of examinations in longitudinal studies, the suggested tolerances of earliness and lateness in days increase as the children become older, on the grounds that elapsed time since birth is increasing too. Irrespective of elapsed time since birth it is the *incremental* period which matters, and these "allowable" tolerances are very high.

Secondly, in a discussion of the assessment of skeletal maturity the authors show the main current methods to be either radiographic evaluation by comparative inspection with 'atlases,' or the award of scores in points to visible centers of ossification appearing on radiographs. They note that the commonly found deviations in order of appearance of these centers complicate and bias the 'points' systems. In fact, such systems must, and do, award points for shape changes as well as appearances, and they are designed to avoid biases from deviant sequences of ossification. These deviations probably complicate the atlas methods to a greater extent.

But such controversies merely whet the appetite and make the reader desirous of discussion and looking up the many references. Perhaps it should be remembered that the majority of growth research has emanated from the United States, and that readers in other countries may well be stimulated and helped by this publication.

The book becomes a little unbalanced by the fact that one fifth is devoted to statistical and graphical reporting methods. *Not* that the subject's extreme importance to investigators is minimized by the reviewer. On the contrary it is perhaps the most important facet to consider first. But this specialized and controversial subject perhaps does not merit such prominence in this particular publication. All are surely agreed to move warily in those areas with a long suffering biometrist always at one's elbow.

The publication is up to the usual excellent standard of the publisher.

It should have a permanent place in many departments which envisage entering the nettly field of human growth.

FRANK FALKNER

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A Guide-Outline for the Study of Physical Growth in Children. By WILTON MARION KROGMAN. 99 pp. Philadelphia Center for Research in Child Growth, 1958. (Mimeographed; published in a limited edition and no longer available).

Anthropologically speaking, Wilton Marion Krogman has been a public benefactor for years, providing bibliographies, compendia and summary-accounts to the advantage of all. "Look it up in Krogman" is good advice, a time-saver, imperative in the smaller libraries, and still true for his latest *Guide-outline for the Study of Physical Growth.*

However, an outline that is more than a reference-list presents particular problems. There must be words between the citations, or they read like a directory of lawyers. The words must say more than "Jones and Jones wrote an article" (which is self-evident), they must guide, especially in a *Guide.*

Krogman seems to have trouble at times in choosing the exact words. Right at the beginning of the guide he describes biochemistry, physiology and psychology as "non-biological areas." Too many references are designated as "best" and in one admittedly difficult reference to articles by A. H. Washburn, he paints them as "a general overall more-or-less philosophical approach to conceptual thinking." The Fels Composite Sheet is incorrectly described as "rating scales," Sendroy's approach to surface-area estimates as "useful," a description like "definitive" quite overused in this booklet. Investigative, summary and discussion papers are lumped as "studies," leaving the user of the guide to discover which.

Lest the reader be misled, Krogman's guide-outline is useful, valuable and unmatched. The arrangement and divisions are reasonable and the formidable problem of sandwiching words between references reasonably well met. Nevertheless, one wishes that he would add criticism, analysis and interpretation to the bare bones of this outline. Since some of the citations are unimpressive, some of the research poorly-conducted, but were properly included because of their uniqueness, it would be helpful to all readers if W. M. Krogman, who stands above reproach, would say so.

STANLEY M. GARN

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Behaviour & Physique: An Introduction to Practical and Applied Somatometry. By R. W. PARNELL. viii + 134 pp. Williams & Wilkins, Baltimore, 1958. $6.80.

The relationship of physique to temperament, and to disease, has fascinated man for centuries. It is found at the heart of the classical descriptions handed down to us in the great writings of clinicians of the past.

The work of W. H. Sheldon at Columbia University, with his first monograph in 1940, greatly stimulated research in this field. Doctor Parnell has reacted to this stimulus by working for several years on the subject at Oxford. This book is a record of his labors.

Doctor Parnell has used the Sheldonian principles of standardized photography and anthroposcopy, and added some direct measurements. His classification is based in the main on the assessment of the relative amounts of muscle, bone and fat in the body, and the linearity of the whole body frame. He has applied this 'somatometry' to samples of children and adults in reference to problems in the fields of education, mental health and genetics. It is here that we see the great need for really basic theory on this whole subject. The past-made criticisms of these and allied methods still hold: truly *independent* measures of fat, muscle, bone, mineral, water (a much neglected and vital component) and surface are first needed before this work can progress much further. Until we can truly assess total body composition (and let us emphasize that this is rarely a static entity) we cannot test the many hypotheses that are so impatiently waiting, nor the methods we might well be able to use in a reasonably simple way in the field. In addition it is a commonplace to say that our psychological colleagues and educators are probably having an even more difficult time assessing personality characteristics and abilities in truly objective ways. The reviewer never ceases to be amazed at, nor to admire, the way skilled independent observers of 'somatotype' photographs agree in their assessments of some components of body build to a remarkably high degree. But, particularly in the light of recent critical discussions and energetic work in the areas of estimations of body composition, we have to then ask: what are they actually assessing?

This book makes it crystal clear that the subject is one of the most fascinating in Human Biology. The author points to the shortcomings of the techniques used and reinforces the stimulation provided by Sheldon. For this reason alone it is valuable indeed.

FRANK FALKNER

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BOOK NOTICES

Techniques of Population Analysis. By George W. Barclay. xiii + 311 pp. John Wiley, New York, 1958. $4.75. An introduction to demographic methods, concerned with the basic concepts (rates, sources of error) and measurements of mortality, fertility, growth of population, migration, and allocation of manpower.

The Biological Way of Thought. By Morton Beckner. viii + 200 pp. Columbia University Press, New York, 1959. $6.00. "Examination of the parts of biological theory that are magnified in the organismic biologist's perspective."

The Blood Pressure in a Population. By Johs. Bøe, Sigurd Humerfelt, and Frøystein Wedervang. 336 pp. A. S. John Griegs, Bergen, Norway. No price indicated. A well documented (66 pp. of tables) study on the relation between height and weight and blood pressure in almost the entire adult population of the city of Bergen.

Perception and Communication. By D. E. Broadbent. v + 338 pp. Pergamon Press, New York, 1958. $8.50. A general view of research on hearing from psychological standpoint, growing out of investigations carried out by the author and centered around the perception of speech.

Bailey's Textbook of Histology. Revised by Winfred M. Copenhaver and Dorothy D. Johnson. xiii + 633 pp. Williams and Wilkins Co., Baltimore, 1958. $11.00. The revision of this standard textbook (1st ed., 1904; 13th ed., 1953) takes into account the recent advances in the study of microscopic structure of tissues and cells (histochemistry, electron microscopy) and places increased emphasis on the physiological significance of structure.

Physiologie des Bewusstseins in entwicklungsgeschichtlicher Betrachtung. By U. Ebbecke. xi + 211 pp. Georg Thieme, Stuttgart, 1959. $6.40. A lecture on the physiology of consciousness, presented at the 1957 meeting of German physiologists, expanded into a book.

The Physical Foundation of Biology: An Analytical Study. By W. M. Elsasser. x + 219 pp. Pergamon Press, New York, 1958. $4.75. This is a thoughtful theoretical physicist speaking. "The approach taken here was made possible by a threefold development: the theory of automata (often designated as computers), the theory of information (mainly developed in communication engineering), and the theory of microscopic measurement in the atomic and molecular domain" (p. ix).

Research with Isotopes in Human and Animal Biology and Medicine. Edited by R. C. Extermann. xxi + 763 pp. Pergamon Press, New York, 1958. $22.50. Third volume of the series "Radioisotopes in Scientific Research" consisting of papers presented at the 1957 international conference held in Paris under the auspices of the UNESCO. Abstracts published in

English, French, Russian and Spanish. Emphasis, as would be natural, is on animal experimentation.

REPRODUCTION AND INFERTILITY. Edited by F. X. Gassner. R. Jensen, H. J. Hill, Assoc. Eds. vi + 267 pp. Pergamon Press, New York, 1959. $6.50. Proceedings of the third symposium devoted to the complex biological problems of reproduction, with two papers dealing specifically with man (maintenance of pregnancy with progestational compounds, classification of male hypogonadism).

RADIOGRAPHIC ATLAS OF SKELETAL DEVELOPMENT OF THE HAND AND WRIST. By William W. Greulich and S. Idell Pyle. xvi + 255 pp. Stanford University Press, 1959. $15.00. Second edition of the outstanding compendium of skeletal age standards (31 plates for males, 29 for females), with a section of the rationale and technique (pp. 1-60), and on "Maturity Indicators of Individual Bones and Epiphyses" (pp. 185-228).

INITIATION MATHÉMATICAL À LA PHYSIQUE MÉDICALE ET À LA BIOLOGIE. By Julien Guelfi. viii + 220 pp. Masson, Paris, 1958. 4,000 fr. A brief, well written vademecum covering all the relevant major facets of mathematics, including probability.

STATISTICS OF EXTREMES. By E. J. Gumbel. xx + 375 pp. Columbia University Press, New York, 1958. $15.00. In biology problems for which statistics of extremes is relevant do not abound. Yet, they are there (oldest age in population statistics, e. g.). Gumbel's volume is a unique methological guide to their treatment.

BIOLOGICAL LABORATORY DATA. By L. J. Hale. x + 132 pp. John Wiley, New York, 1958, $2.75. Selected information on mathematical and statistical formulae, weights and measures, and other matters (physics, chemistry, histological techniques) of interest to the biologist.

THE FAMILY AND POPULATION CONTROL: A PUERTO RICAN EXPERIMENT IN SOCIAL CHANGE. By Reuben Hill, J. Mayone Stycos, and Kurt W. Back. xi + 481 pp. University of North Carolina Press, Chapel Hill, 1959. $8.00. A study of population control viewed as a phenomenon of family planning, focused on the paradox of widespread acceptance of the ideal of a small family size and the absence of decline in fertility, ending with recommendations for government program of action.

RUSSIAN-ENGLISH MEDICAL DICTIONARY. By Stanley Jablonski. Edited by Ben. S. Levine. xi + 423 pp. Academic Press, New York, 1958. $11.00. The purpose of the volume is "to fill a long-standing need for a specialized tool designed for the English translation of Russian medical literature." Well filled! Published with material assistance from the Russian Scientific Translation Program, U. S. Public Health Service.

PRATIQUE DU LABORATOIRE. Edited by Ch. Jaulmes, A. Jude, J. Quérangal des Essarts. 872 pp. Masson, Paris, 1958. 7.600 fr. A compendium of medical laboratory procedures. For the readers of this journal the section on blood groups (p. 543 f.) may be especially relevant.

THE NEGRO PERSONALITY. By Bertram P. Karon. vii + 184 pp. Springer Publishing Co., New York, 1958. $4.50. "Combining the statistical control of repeated samples with the depth of a projective technique, Dr. Karon illuminates the world which the white American has created for the colored American." (from S. S. Tomkins' forward).

PHYSIOLOGY OF MUSCULAR ACTIVITY. By Peter V. Karpovich. xvii + 368 pp. W. B. Saunders Co., Philadelphia, 1959. $5.50. Fifth edition of the old standby, originally written by E. C. Schneider, with a chapter on the relation of body type and posture to physical fitness.

NUTRITION AND ATHEROSCLEROSIS. By Louis N. Katz, Jeremiah Stamler, and Ruth Pick. 146 pp. Lea and Febiger, Philadelphia, 1958. No price indicated. A comprehensive review (close to 800 references) and appraisal of the rapidly growing field of research on the significance of nutrition in the development of atherosclerosis.

THE MEDICAL WORLD OF THE 18TH CENTURY. By Lester P. King. xvii + 346 pp. University of Chicago Press, Chicago, 1958. $5.75. "A series of loosely related essays dealing with phases of the medical scene that are generally neglected in the formal histories" (Ilza Veith, in the Forword).

SYLLABUS IN ROENTGENOGRAPHIC CEPHALOMETRY. By Wilton Marion Krogman and Viken Sassouni, 366 pp. Philadelphia Center for Research in Child Growth, Philadelphia, 1957. No price indicated. An inventory of the literature (22 pp. of references), evaluation of technic and X-ray film interpretation (landmarks, measurements) plus appraisal of relation to collateral sciences.

MENTAL SUBNORMALITY. By Richard L. Masland, Seymour B. Sarason, Thomas Gladwin. 442 pp. Basic Books, New York, 1958. $6.75. A survey of research on biological, psychological, and cultural factors, sponsored by the National Association for Retarded Children.

CHEMICAL BASIS OF DEVELOPMENT. Edited by William D. McElroy and Bentley Glass. xi + 934 pp. Johns Hopkins Press, Baltimore, 1958. $15.00. A symposium held at Johns Hopkins University in honor of Professor B. H. Willier, distinguished teacher and investigator in animal development, a field encompassing a wide range of problems and requiring participation of various disciplines, including embryology, biochemistry, and genetics.

LOOKING AT CHROMOSOMES. By John McLeish and Brian Snoad. vii + 87 pp. St. Martin's Press, New York, 1958. $3.75. By text, drawings and—most importantly—a series of photographs the authors portray the behavior of chromosomes in plant cells, emphasizing that the mechanisms underlying heredity are fundamentally similar in plants and animals.

MODERN SCIENCE AND THE HUMAN FERTILITY PROBLEM. By Richard L. Meier. xiii + 263 pp. John Wiley, New York, 1959. $5.95. Centered around the potential breakthrough in fertility control by means of oral contraceptives and the analysis of social consequences of scientific discovery.

SOCIOLOGY TODAY. Edited by Robert K. Merton, Leonard Broom, and Leonard S. Cottrell, Jr. xxxiv + 623 pp. Basic Books, New York. $7.50. The three editors and 27 contributors set out "to sort out and critically examine problems for investigation that developments in the major branches of sociology have thrust into prominence."

SELECTED STUDIES OF MIGRATION SINCE WORLD WAR II. 244 pp. Milbank Memorial Fund, New York, 1958. $1.00. Migration is considered both within the international context and in the U. S. setting, including theoretical (models in migration) as well as economic aspects.

BIOLOGY OF HAIR GROWTH. Edited by William Montagna and Richard A. Ellis. xvii + 520 pp. Academic Press, New York, 1958. $15.00. Spanning the life cycle (Ch. 1 Embryology of hair; Ch. 20 Aging of the human male scalp), combines the morphological and functional methods and viewpoints, including histochemistry and electron microscopy. Includes "Age, sex and genetic factors in the regulation of hair growth in men" (Ch. 16).

RECOMMENDED DIETARY ALLOWANCES, 36 pp. Publication 589, National Academy of Sciences—National Research Council, Washington, D. C., 1959. $1.00. A 1958 revision of the standard guide concerning nutrient allowances "for the maintenance of good nutrition in healthy persons in the U. S. A."

FEDERAL FUNDS FOR SCIENCE. VII. The Federal Research and Development Budget Fiscal Years 1957, 1958, and 1959. National Science Foundation, iii + 78 pp. The U. S. Government Printing Office, Washington, D. C. $0.45. Analysis of the volume of funds provided for research and development through the Federal budget.

FUNDAMENTALS OF ECOLOGY. By Eugene P. Odum. xvii + 546 pp. W. B. Saunders Co., Philadelphia, 1959. No price indicated. Two chapters are devoted specifically to applications of human ecology but other sections, e. g., the treatment of radiation (Ch. 14), are also relevant.

HORMONE UND PSYCHE: DIE ENDOKRINOLOGIE DES ALTERNDEN MENSCHEN. (Hormones and Psyche: The Endocrinology of the Aging Man.) Edited by H. Nowakowski. vii + 355 pp. Springer-Verlag, Berlin, 1958. No price indicated. Symposium of the German Society for Endocrinology. Clinically oriented.

THE VERTEBRATE STORY. By Alfred Sherwood Romer. vii + 437 pp. University of Chicago Press, Chicago, 1959. $7.00. A well told account of the structure, function and ways of life of the backboned animals, with the evolutionary story as the leading theme. The last three chapters deal with primates, including man.

DIFFERENTIAL FERTILITY IN BRAZIL. By J. V. D. Saunders. x + 90 pp. University of Florida Press, Gainesville, Fla., 1958. $3.50. Analysis of the rates at which various residential, racial, regional and other (occupational, religious, socioeconomic) groups reproduce in a nation with a tremendous development potential.

AGGRESSION. By John Paul Scott. xi + 149 pp. University of Chicago Press, Chicago, 1958. $3.75. Aimed at the causes of aggressive behavior in the animal kingdom, the volume clarifies the implications for human behavior.

LE PLACENTA HUMAIN: ASPECTS MORPHOLOGIQUES ET FONCTIONNELS. By Prof. Jean Snoeck. 718 pp. Masson, Paris, 1958. 7.500 fr. An outstanding monograph on the morphology, biochemistry and physiology of human placenta by the research team of the Clinic of Gynecology and Obstetrics, Free University of Brussels.

SONDERAUSSCHUSS RADIOAKTIVITÄT BUNDESREPUBLIK DEUTSCHLANDS. 68 pp. Georg Thieme, Stuttgart, 1958. $1.10. The first report of the West German Committee on Radioactivity. Of special relevance is Ch. 3 dealing with the appraisal of the radiation load on the population.

CLIMATOLOGY: REVIEWS OF RESEARCH. UNESCO. 190 pp. UNESCO Publication Center, New York, 1958 paper. $5.00. Of special interest is D. H. K. Lee's chapter on the microclimates (climatic circumstances affecting an individual at a given moment, "proprioclimates") of man and domestic animals.

HUMAN AND ANIMAL ECOLOGY: REVIEWS OF RESEARCH. 244 pp. UNESCO Publication Center, New York, 1957. No price indicated. Two out of the seven papers deal with the effects of environment in arid regions on man (human communities, the biology of man).

GENERAL SYSTEMS. Vol. 3. Edited by Ludwig von Bertalanffy and Anatol Rapoport. Society for General Systems Research, Ann Arbor, Mich., 1958. No price indicated. The third yearbook of the Society of General Systems. The bulk of the volume is devoted to biology and, in particular, to evolution and to population dynamics.

WENNER-GREN FOUNDATION FOR ANTHROPOLOGICAL RESEARCH, INC. Report on the Foundation's Activities for the Year Ended January 31, 1958. 90 pp. Report of activities and publications resulting from research supported by the Foundation. Substantial amount of financial help was allocated to physical anthropology.

AVIATION MEDICINE. Edited by Clayton S. White, W. Randolph Lovelace, II, and Frederic G. Hirsch. vii + 305 pp. Pergamon Press, New York, 1958. $9.80. Assessment of recent advances in methods dealing with selected topics, including spirometry, temperature measuring techniques, and uses of high-speed motion-picture photography.

THIRST: PHYSIOLOGY OF THE URGE TO DRINK AND PROBLEMS OF WATER LACK. By A. V. Wolf. x + 526 pp. Charles C. Thomas, Springfield, Ill., 1958. $12.50. A veritable storehouse of physiological and medical knowledge about water intake, including consequences of sea water drinking. In the second part (p. 371-422) the laboratory, clinical and field studies are supplemented by classical accounts of the effects of "real-life" water deprivation.

NEUROLOGICAL BASIS OF BEHAVIOR. Edited by G. E. W. Wolstenholme and Cecilia M. O'Connor for the Ciba Foundation. xii + 400 pp. Little, Brown, and Co., Boston, 1958. $9.00. Proceedings of an outstanding symposium, multidisciplinary in nature.

THE MEASUREMENT OF COLOR. By W. D. Wright. ix + 263 p.. The Macmillan Company, New York, 1958. $10.75. Competent presentation of the principles, methods, and applications of the trichromatic system of color measurements.

THE BIOLOGY OF AGING. Edited by W. G. Yapp and G. H. Bourne. xiv + 128 pp. The Institute of Biology, London, 1957. $3.50. At least one half of the volume is likely to be of direct interest to readers of *H. B.* (demographic aspects, longevity, working capacity, nutrition).

JOSEF BROŽEK

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INFORMATION FOR CONTRIBUTORS

Human Biology, a record of research, is a quarterly journal devoted to the subject its name implies. It publishes original articles dealing with human genetics, development, growth, reproduction and aging, functional anatomy, bio-anthropology, human ecology, population dynamics, etc.

The original manuscripts, typed double-spaced on one side of 8½ x 11 inch paper in complete and finished form precisely as they are to appear, should be sent—packed flat—to the editor, Dr. Gabriel Lasker, Wayne State University College of Medicine, 1401 Rivard Street, Detroit 7, Michigan. It is desirable that a second copy be included in order to expedite prompt consideration, and that an additional carbon copy and copies of all illustrations should be retained by the author.

Where possible, illustrations should be made for reproduction as line engravings, hence drawn in India ink as boldly as possible. Lettering must remain legible after figures are reduced to fit on 4¼ x 6¾ inch pages (letters 1½ to 2 mm high after reduction are adequate). Figures should be numbered consecutively with Arabic numerals. Tables should be numbered independently from 1 up, and both figures and tables should have captions. When the amount of tabular and illustrative material is judged to be excessive or unusually expensive, authors may be requested to pay the excess cost. Galley proofs will be sent to the author and must be returned promptly; when the author makes substantial departures from copy, the cost will be charged to him. Three copies of an abstract (not to exceed 225 words) should be submitted when the proof is returned. One will be sent to the editor of *Biological Abstracts*.

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THE ESTIMATION OF LEAN BODY WEIGHT FROM "SKELETAL" MEASUREMENTS

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IT has been established by *in vitro* analysis that the proportions of water, mineral and organic solids are remarkably constant in the adult, fat-free body. In lower animals, specifically in in-bred strains such as the rat, the nearly exact ratio of water to solids in all individuals of the in-bred group allows one to calculate with a high degree of accuracy, from total body water alone, the amount of organic and mineral solids present (Babineau and Pagé, 1955). It is probable also that a fourth entity, the essential lipids chiefly in bone marrow and the central nervous system, constitute a rather uniform percentage by weight of the more inclusive *in vivo* entity, the lean body mass (LBM), (Behnke *et al.*, 1953).

The fat-free body, as well as the lean body mass, may serve as reference bodies. In man, both whole body density and total body water (TBW) determinations permit an estimate to be made, either of fat-free weight or of the lean body mass. In addition, one may consider the suitability of measurements of bone diameters as a basis for the estimation of skeletal weight and, in turn, lean body mass. It appears that the external size of the skeleton, despite involutional changes in the cortex with age, remains constant throughout the active period of adult life in healthy individuals. In the disease state (not involving bone), particularly in the presence of edema, it would be advantageous (since total body water and density techniques may not reveal the desired information or be feasible) to employ bone measurements, provided that they can be translated into bone or reference body weights.

In a previous paper (Behnke *et al.*, 1959), the body was compared with a cylinder, and girth dimensions, principally, were converted into a "body radius." Thus, R, the body radius for any individual, was obtained from D (the anthropometric dimensions) divided by a constant (k). Body weight was calculated from $\pi R^2 h$, h representing stature in decimeters. In this paper the same type of analysis will be applied to skeletal dimensions in an effort to translate "skeletal" measurements into their LBW equivalents in much the same manner as the "envelope" measurements and stature were used to compute gross body weight. In this connection, one may consider the intermediate step involved in the estimates of the weight of the skeleton itself from anthropometric measurements. Matiegka (1921) stated that skeletal weight could be determined from 8 transverse diameters ($8TD$) measured by means of calipers applied to the condyles of the femur and humerus, and at the wrists and ankles. His formula is an expression of the basic principle underlying our analyses, namely

$$\text{Bone weight (gm)} = \left(\frac{8TD}{n}\right)^2 \times h \times k$$

in which h is stature in cm, and the constant k was presumed to lie in the range of 1 to 1.2. It was "to be determined subsequently." However, although Matiegka's supporting data were inadequate, it can be shown that skeletal weights calculated from his formula are supported by estimates of skeletal size obtained by indirect methods and by the occasional direct analysis.

Trotter (1954) made the measurements suggested by Matiegka, as well as measurements from roentgenograms, of the area of the femur and of the compact bone in the middle half of the femur on 24 white male skeletons. Subsequently Merz, Peterson and Trotter (1956) examined some 200 skeletons and developed equations for the estimation of the weight of the skeleton from x-ray visualization of the femur shaft. For our purpose, the value of this material is somewhat limited by the fact that the bones were degreased by exposure to fumes of benzol to remove most of the fat and also water by drying. Hence, the weight of the fresh bone is not known. Further, the skeletons were chiefly from older individuals (average age 55, range 18 to 87 years). However, from an analysis of Trotter's data, it was apparent that a reasonably good correlation ($r = .89$) existed between skeletal diameters (Matiegka) and the planimeter x-ray area of the femur. The area of compact bone of the middle third of the femur, together with stature, showed the highest correlation with actual bone weight, in Trotter's analysis, $r = \sim .82$. The highest correlation, $r = .96$, was found between the weight of the femur and the weight of the entire skeleton. These relationships make possible helpful deductions as to skeletal size from the characteristics of a single bone which can be studied in its entirety on the x-ray film.

Estimates of body weight from bone dimensions. Willoughby (1932) recorded certain "skeletal" dimensions on a selected group of athletic young men, and also on young women, as did Turner (1943) on college girls, from which they calculated "optimal" weights. The measurements were mainly diameters, *i. e.*, biacromial, chest, bi-iliac, and bitrochanteric, but included also circumferences of the wrist and ankle. To these measurements, we have added diameters of the knee, wrist and ankle.

In this paper are reported the results of anthropometric "skeletal" measurements and those also of the trunk and extremities (Matiegka's $8TD$), utilizing in this case an x-ray technique.

PROCEDURES

The volume of the body was determined in a sealed chamber by the helium dilution technique (Siri, 1956). Siri's equation for estimating the percentage of fat from density alone is: $\% \text{ fat} = \frac{495.0}{\text{density}} - 450.0$. In this equation, the constants for the density of the fat-free body and of pure fat are 1.100 and 0.90 respectively.

Total body water was determined as the "tritium" space using tritiated water which was taken by mouth. Samples of urine were collected subsequently after equilibrium had been established between the tritiated water and body fluids. The standard error of estimate in replicate analyses by this technique is ± 2% of the body water measured.

The weight of the fat-free body may be estimated from total body water (kg), substituted in the following expression:

$$\text{F-FB} = 100\left(\frac{\text{TBW}}{72}\right).$$

The combined equation (Siri) incorporating both body density and percent body water for the estimation of the percentage of fat in the body is:

$$\%\ \text{fat} = \frac{213.66}{\text{density}} - 77.49\left(\%\ \frac{\text{TBW}}{\text{weight}}\right) - 137.4.$$

This equation essentially permits an estimate of fat to be made from body density in which the double component system comprises fat and the fat-free mass minus the separately analyzed body water. From theoretical considerations, the standard error of estimate when body fat is calculated from either density or TBW alone can be shown to be of the order of ± 4% of body weight. This is reduced to ± 2% if the calculation of body fat is made from both TBW and density, as in the combined Siri equation.

Anthropometric measurements (31 subjects).

The technique employed was essentially that outlined by Hertzberg and Daniels (1954). A broad blade (Hrdlička) anthropometer was employed for the measurement of chest width only; all other diameters were measured with a narrow blade (Martin) anthropometer. A pliable plastic tape was used for girth measurements of the wrist, ankle, and knee. The details of the measurements follow, since the data pertain only to the anatomical locations selected, and the procedure as outlined.

With the subject in the sitting position, arms at the sides and forearms (palms down) resting on the legs, the following measurements are made: 1) biacromial width, the distance between the most lateral projections of the acromial processes (observer standing behind the subject); 2) knee-to-knee width, the maximal horizontal distance between the lateral surfaces of the knees in contact; 3) knee width, the distance between the apex of head of fibula and the median condyle of the tibia.

With the subject standing, feet together: 4) chest width, level of the nipple line and with the breath held at the mid-point of a *normal* tidal volume excursion (moderate pressure is exerted on the broad blade anthropometer); 5) bi-iliac width, maximal diameter between iliac crests, pressure is applied to the blades of the anthropometer to feel bone resistance of the cristal borders; 6) bitrochanteric width, maximal diameter between the greater trochanters with pressure applied to the narrow blades of the anthropometer; 7) wrist width, distance between styloid processes of radius and ulna; 8) wrist circumference, minimal girth measured *proximal* to the styloid process of the ulna.

With the subject standing on a bench: 9) ankle width, distance between maleoli with the bar of the sliding caliper held perpendicular to the long axis of the foot; 10) ankle circumference, minimal leg girth above maleoli; 11) knee circumference, with the knee slightly flexed, the tape is drawn across the middle of the patella.

Paired right and left side measurements are made on the extremities and the average recorded.

X-ray technique (22 subjects).

a. Extremities (figure 1). The radiographs were made with standard technique, being careful to center the tube vertically above the middle of the part. The distance, tube to table top, was about 40 inches. Instead of calculating the magnification, a lead ruler, notched at 5 cm intervals, was laid on a wood block alongside the part half-way between upper and under surfaces. When a Bucky diaphragm was used, the correction approached 10%. It was convenient to take both knees on one end of a large film and both ankles on the other. The feet should be tied together to prevent foreshortening by external rotation. Similarly, both elbows and both wrists were taken on one film.

b. Trunk. The chest and the pelvis were taken at 6 ft. distance, with subject standing, facing the tube. The lead ruler was on a stand at his side, halfway between front and back. The subject held his breath with lungs about half full.

The diameters and their end-points were inscribed on the film (figure 1), and then measured to the nearest half millimeter, using dividers and millimeter scale.

Since we have become more careful to avoid unnecessary x-ray exposure, we now recommend that the pelvis be taken with subject supine, using a 14 × 17 inch film transversely at about 40-inch tube distance. With a long cone to cover a 4 or 5 inch circle, the tube is brought over one iliac crest and then over the other, the two exposures being made on one film. This gives a record without magnification. The gonads receive only a negligible exposure, being outside the useful beam. There is no need to use such a technique on the chest, for the gonads are outside the beam.

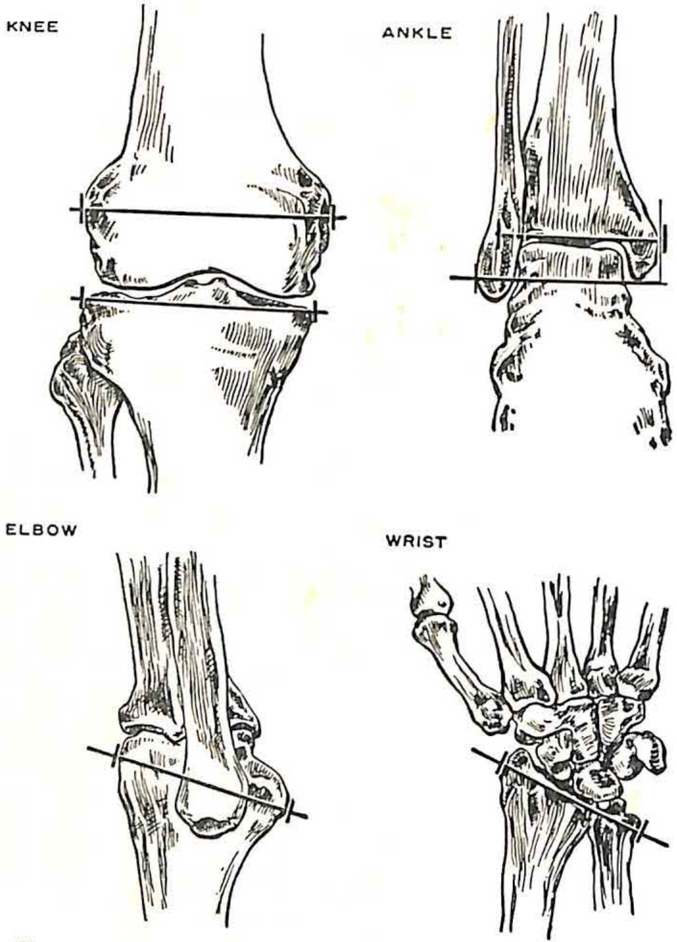

FIG. 1. SITES OF MEASUREMENTS ON X-RAY FILM OF DIAMETERS OF DISTAL ENDS OF LONG BONES (8*TD*) AND OF THE PROXIMAL AND DISTAL ENDS OF THE TIBIA. THE BI-MALLEOLAR DIAMETER IS MEASURED ON A HORIZONTAL PLANE AS INDICATED.

DERIVATION OF CONSTANTS FROM GROUP DATA

The 31 subjects were selected so as to provide a wide range of statures and gross weights. This is reflected in the high coefficients of variation (C. V.) for stature (4.9%) and for weight (18.1%) compared with the C. V. values for the same parameters in the group of Hertzberg and Daniels (1954), table 1. The mean body weight for the Navy group of 31 men was 78.3 kg, and mean stature, 17.83 dm. The mean group value for fat-free weight calculated from Siri's combined equation was 61.6 kg, which was slightly less than the median value of 62.9 kg.

TABLE 1

Mean values (cm) for stature, "skeletal" diameters and circumferences of the ankle and wrist of young men

GROUP	STATURE	WEIGHT	BIACROM.	CHEST	BI-ILIAC	BITROCH.	KNEE-TO KNEE	ANKLE	WRIST
Navy* ($N = 31$)	178.3	78.3	40.6	29.9	29.4	32.8	20.5	22.5	17.2
C. V.	4.9	18.1	4.6	6.5	7.8	6.9	5.2	5.6	5.0
Air Force** ($N = 4000$)	175.5	74.4	40.0	30.6	—	33.5	20.1	22.7	17.4
C. V.	3.5	12.7	4.7	6.7	—	5.5	6.6	6.4	5.8
Athletes*** ($N = 50$)	175.3	76.2	40.9	30.5	28.4	32.8	—	22.4	18.0

Additional Measurements: (Navy)
Knee W (9.2), Knee C (38.3), Wrist W (5.59), Ankle W (6.94)

Sources: *Behnke (this report), **Hertzberg and Daniels (1954), ***Willoughby (1932).

If lean body weight contains an additional 3% essential fat, then the mean lean body weight $\overline{(\mathrm{LBW})}$ for the Navy group may be estimated as 63.4 kg. The mean radius of the lean body mass, $\bar{R}$ (LBW), was derived from substitutions in the formula

$$R(\mathrm{LBW}) = \sqrt{\frac{\overline{\overline{\mathrm{LBW}}}}{\pi h}} = \sqrt{\frac{63.4}{\pi 17.83}} = 1.064\ \mathrm{dm}.$$

Dividing the mean group "skeletal" measurements by 1.064, one obtains the "k" values listed in table 2. The calculation of lean body weights of subjects 2 and 4 is presented also in this table. Thus, the radius of the lean body mass for any individual, $R(\mathrm{LBW})$, is calculated from the ratio, $\frac{\mathrm{Skel\ I}}{k}$. The value of k for this combination of measurements is 181, and this is the sum of the "k" values for the single dimensions in table 2. LBW for the individual is then calculated from $\pi R^2(\mathrm{LBW})h$, where h is the individual's stature in decimeters. The dimensions listed in table 2 are designated Skel I. With the exception of chest width and

TABLE 2

Derivation of Navy group constants ("k" values) for the calculation of lean body weights from anthropometric dimensions and application of these constants to data on two subjects of different build

NAVY GROUP ($N = 31$) Stature (h) = 17.83 dm			SUBJECT 2 $h = 17.88$ dm			SUBJECT 4 $h = 18.03$ dm		
DIMENSIONS (D) (Skel I)	D (Mean Values)	"k" Values $D \div 1.064$	D	Single "R" Values $D \div$ "k"	% dev. from R(LBW)	D	Single "R" Values $D \div$ "k"	% dev. from R(LBW)
Diameters	cm		cm	dm		cm	dm	
biacrom.	40.6	38.2	42.1	1.102	—3.3	41.8	1.094	—3.4
chest	29.9	28.1	32.3	1.149	+0.8	32.7	1.164	+2.7
bi-iliac	29.4	27.6	31.5	1.141	+0.1	33.6	1.217	+7.4
bitroch.	32.8	30.8	34.8	1.130	—0.9	34.6	1.177	+3.9
knee-to-knee	20.5	18.9	22.4	1.185	+3.9	20.3	1.074	—5.2
Circumferences								
wrist	17.2	16.2	19.4	1.198	+5.1	16.6	1.025	—9.5
ankle	22.5	21.1	23.8	1.128	—1.1	25.6	1.213	+7.1
Σ	192.9	180.9	206.3			205.2		

$k = 180.9$ (~ 181)

$\bar{R}(\text{LBW}) = 1.064$

LBW (median) $= \pi \bar{R}^2(\text{LBW})h = 63.4$

$R(\text{LBW}) = \frac{206.3}{181} = 1.140$

LBW $= \pi R^2(\text{LBW})h = 73.0$

$R(\text{LBW}) = \frac{205.2}{181} = 1.133$

LBW $= \pi R^2(\text{LBW})h = 72.7$

the substitution of knee girth for the diameter of both knees, these dimensions have been employed extensively by Turner (1943) for the calculation of "optimal" weights of college girls.

TABLE 3

Derivation of x-ray "skeletal" dimensional values and dimensional (lean body weight) constant (k) for a military reference man from Navy (roentgenogram) measurements, and cadaver bone anthropometry

Dimensions (cm) "8 transverse diameters" ($8TD$)†	DATA SOURCE					
		X-ray Navy Men $N = 22^*$	X-ray Navy Men $N = 38^{**}$	Anthropometric Disarticulated Male Skeletons $N = 20^{***}$	REFERENCE MAN	
	R(dm)	1.064	1.071	1.055	1.063	$k(D(\text{cm})) \div R$
Elbows		12.8	13.2	"8 transverse diameters"	12.93	12.16
Wrists		9.8	9.9		9.90	9.31
Knees		17.2	17.7		17.39	16.36
Ankles		12.8	13.1		12.93	12.16
$\Sigma 8TD$		52.6	53.9	53.0	53.15	
$k = \Sigma 8TD \div R$		49.0	50.4	50.2	50.0	49.99
Tibia (intercondyles)		15.8	16.4	—	15.96	15.01
Tibia (distal end)		11.0	11.4	—	11.11	10.45
Age		29 (20-53)	29 (19-42)	55 (18-87)		
Stature dm		17.77	17.63	17.15		17.40
$\bar{m}$ Weight kg		78.3	75.1	—		70.0
LBW ($\pi R^2 h$)		63.4	63.4	60.0		61.8
LBW (median value) $=\Diamond=$		$0.201\bar{m}h^2$	$0.204\bar{m}h^2$	$0.204\bar{m}h^2$		$0.204mh^2$

† Elbow, knee (bicondylar: humeral, femoral diam.); wrist (radial-ulnar styloid diam.); ankle (bi-malleolar diam.).

Sources: *Behnke (this report), **Garrison (1957), ***Trotter (1954).

Another combination of measurements was also selected so that the trunk and extremities would have approximately equal weighting in the computation of lean body weight. These have been designated Skel II and, in addition to the biacromial, chest, and bitrochanteric diameters, include the following combinations of dimensions for which the "k" values are given in parentheses: bi-knee diameter plus the average of the right and left knee diameters (28.7), the ankle circumference plus the average of the right and left ankle diameters (27.7), and the wrist circumference plus the right and left wrist diameters (25.7). The summated "k" values for the Skel II group are equal to 179.2, rounded to 179 for the calculation of individual weights.

Constants for the diameters of single dimensions not previously given are: knee (8.66), wrist (5.31), ankle (6.52), and for the circumference of the knee, 36. The mean group value in centimeters for any dimension or combination of dimensions is obtained from the product of the "k" value and mean group radius, 1.064.

Derivation of constants for the x-ray film measurements.

The mean group values for measurements of the "8 transverse diameters" (8*TD*) from roentgenograms are listed in table 3. Included in this table also are the anthropometric measurements of Trotter (1954) of the 8 transverse diameters shown in figure 1. The technique used by Garrison (1957) was the same as that described in this report. It is seen from the table that the summated "k" values (~ 50.0) are nearly the same for the three groups and serve to define the relationships for the Reference Man. The derivation of median values for lean body weights for the Garrison and Trotter data will be discussed in a subsequent paragraph. In addition to the constants applicable to the extremity diameters, there may be added the constants obtained from roentgenograms for the lateral diameter of the chest (26.22), the bi-iliac diameter (25.94), and the bitrochanteric diameter (28.28). The actual mean values in centimeters for these dimensions may be calculated from the product $k \times 1.064$.

ANALYSIS OF DATA

Fat-free body weight.

As previously stated, the mean value of fat-free weight (F-FW) for the 31 men, calculated from both density and TBW determinations (Siri combined equation) was 61.6 kg. The F-FW computed from density alone was 60.1 kg, and from TBW, 63.8 kg. These mean values are only slightly altered when the group size is reduced to 22 men (table 4). The mean value for TBW of 46.0 kg is 76.5% of the fat-free body weight calculated from the density values. This is considerably higher than the values of 72 to 73% obtained on other groups of men (Osserman *et al.*, 1950), and from analyses of animal carcasses (Babineau and Pagé).

TABLE 4

Lean body weight (LBW) in kg, estimated from body density, water determinations, and from x-ray and anthropometric measurements ($\pi R^2 h$)

				X-RAY		ANTHROPOMETRIC DIAMETERS						FAT-FREE WEIGHT from	
SUBJECT	AGE	STATURE	WEIGHT KG.	$8TD$	Chest W Bi-iliac Bitroch.	Chest W Bi-iliac Bitroch.	Chest Biacrom. Bitroch.	Wrist Ankle Knee	SKEL. I	SKEL. II	LBW	Total Body Water	Body Density
1	34	20.15	117.5	87.9	—	91.3	88.0	89.0	87.7	88.2	77.3	75.7	76.0
2	52	17.88	100.0	70.6	71.7	73.5	71.7	77.7	73.0	73.6	70.4	68.9	70.8
22	48	17.75	100.0	71.4	—	77.8	78.5	71.5	75.5	74.4	74.8	80.0	64.7
4	31	18.03	98.9	62.3	65.7	77.5	71.5	58.9	72.7	68.2	65.0	67.1	61.2
13	39	18.80	98.4	76.0	77.2	80.2	76.5	71.9	76.4	75.5	75.1	74.3	72.8
21	21	17.75	77.0	66.3	71.6	68.3	67.3	68.1	66.1	66.9	66.5	70.3	60.2
24	31	19.35	83.9	74.2	75.0	72.0	71.4	71.1	72.9	73.3	69.4	67.1	67.0
20	31	17.73	75.9	58.4	64.4	59.3	58.0	60.9	58.0	57.9	64.9	66.7	60.6
3	21	17.93	67.7	62.7	70.2	66.2	64.4	64.0	63.8	62.9	64.3	62.9	62.9
20	20	16.20	61.1	53.8	52.6	50.4	52.2	52.2	52.6	52.7	51.6	50.4	51.5
17	20	16.10	54.8	48.4	47.5	44.7	47.8	47.8	47.7	49.5	49.5	47.6	50.5
28	23	17.18	52.4	53.5	51.4	47.0	48.1	49.7	48.7	49.3	46.7	45.0	46.7
$\bar{M}$				65.4	—	67.3	66.3	65.2	66.3	66.0	64.6	64.7	62.8
$\bar{M}$ ($N = 22$)				63.7	—	—	63.4	63.4	63.4	63.6	63.4	64.4	60.9
C. V.				11.8	—	—	12.7	12.3	12.8	11.7	12.2	14.7	11.5

LBW = Fat-free weight ÷ 0.97. Fat-free weight is calculated from TBW and density in Siri's equation.

The differences between F-FW calculated from density and from body water alone are in some individuals exceptionally large, *e. g.*, in subject 22 it amounted to 15.3 kg. If the gross fat-free components of the body, such as water, constitute constant percentages of F-FW, the densitometric and TBW determinations of F-FW should agree. In this group study the variability expressed as the standard error of the differences between the two sets of values is ± 8.2% of the mean lean body weight, *i. e.*, ~ 5 kg. The data (table 4) serve to emphasize the rationale not only of carrying out both density and TBW determinations, but the need to ascertain additional body components, such as exchangeable chloride and potassium in order to resolve the discrepancy between the TBW and density determinations of fat-free weight in some individuals. If the healthy adult fat-free body contains variable percentages of water in different individuals, then the premise underlying our estimates of LBW from skeletal dimensions is not rigidly valid.

Lean body weights calculated from "skeletal" measurements (table 4, fig. 2).

The following tabulation gives the correlation coefficients and the standard errors of estimate (V) expressed as percentages referable to the mean LBW of 63.4 kg.

Dimensions	X-ray	Diameters		$R(e)+R(t)$	SKEL I	SKEL II
	8 *TD*	wrist knee ankle $R(e)$	biacrom. chest bitroch. $R(t)$			
r	.81	.84	.84	.89	.83	.84
V	10.1	9.4	6.8	6.0	9.2	7.0

The most unsatisfactory group of measurements are those which include the bi-iliac diameter. Unusually large discrepancies in some individuals characterize this measurement as revealed by a comparison of anthropometric and roentgenogram data. Variations in the amount of excess fat could account for the largest error in the measurements.

Effect of subcutaneous fat on the anthropometric measurements.

The effect of dietary induced weight loss on subcutaneous fat is reflected in the changes recorded in connection with measurements made on subject 2 (table 5). The Skel II data are less sensitive than are the Skel I values. The desired precision in technique can be made only if the diameter measurements are corrected for thickness of tissue over the bone landmarks.

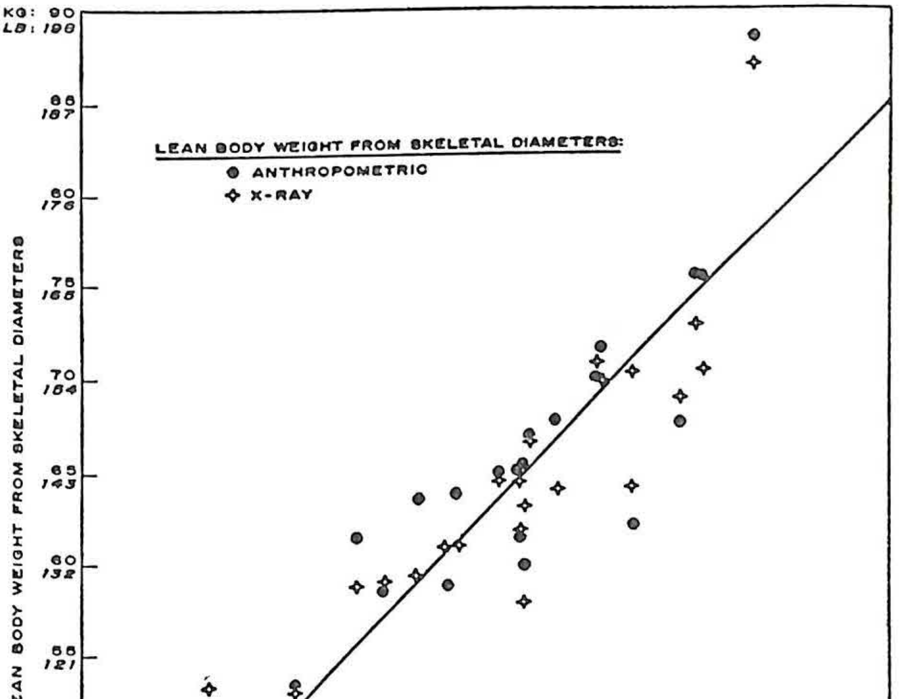

FIG. 2. A COMPARISON OF INDIVIDUAL LEAN BODY WEIGHTS (LBW) CALCULATED FROM ANTHROPOMETRIC "SKELETAL" DIMENSIONS WITH LBW BASED UPON DENSITY AND TOTAL BODY WATER ANALYSES. THE BONE DIMENSIONS MEASURED WITH THE ANTHROPOMETER ARE CHEST W, BIACROMIAL, BITROCHANTERIC, WRIST, KNEE AND ANKLE DIAMETERS. THE DIAMETERS MEASURED ON THE X-RAY FILMS ARE THE 8 TRANSVERSE DIAMETERS OF LONG BONES.

TABLE 5

Successive "skeletal" measurements of subject 2 during a period of voluntary weight (kg) reduction

	1955		1956		
	Sept	Feb	Mar	June 8	June 22
SKEL I					
Biacromial	42.1	41.7	41.3	41.4	41.4
Chest W	32.3	31.4	31.1	31.4	30.9
Bi-iliac	31.5	31.1	30.5	31.4	30.3
Bitroch.	34.8	35.1	34.1	34.2	34.1
Ankle	23.8	23.7	23.3	23.3	23.3
Wrist	19.4	19.1	18.9	19.1	19.2
Knee-to-knee	22.4	21.9	21.8	21.4	21.6
	206.3	204.0	200.9	202.2	200.8
Calc. LBW*	73.0	71.3	69.2	70.1	69.1
Body Weight	100.0	91.6	86.3	86.4	83.6
SKEL II					
Biacromial	42.1	41.7	41.3	41.4	41.4
Chest W	32.3	31.4	31.1	31.4	30.9
Bitroch.	34.8	35.1	34.1	34.2	34.1
Knee-to-knee Av. R & L Knee W	33.1	32.5	32.0	31.4	31.9
Ankle C Av. R & L Ankle W	31.1	31.0	30.8	30.8	30.7
Wrist C R + L Wrist W	31.6	31.8	31.4	31.5	31.9
	205.0	203.5	200.7	200.7	200.9
LBW**	73.6	72.6	70.6	70.7	70.7
W	100.0	91.6	86.3	86.4	83.6

* $\text{LBW} = \left(\frac{\text{Skel I}}{181}\right)^2 \pi h$ ** $\text{LBW} = \left(\frac{\text{Skel II}}{179}\right)^2 \pi h,\ h = 17.88 \text{ dm.}$

Variability of intra-individual "skeletal" measurements.

Intra-individual variation of radii calculated from single dimensions relative to the radius of the LBM is evident from data in table 2. Thus, the trunk dimensions of subject 4 are relatively larger than his extremity dimensions and the inverse pertains to subject 2. Similar differences in girth dimensions were reported also for these subjects (Behnke *et al.*, 1959).

The standard deviations of the differences between a radius calculated from any single dimension and the radius of the lean body mass computed from the summated dimensions are given by the following (±) values: diameters; biacromial (2.87), chest (3.04), bitrochanteric (2.70), knee-to-knee (3.16) and circumferences: ankle (3.62) and wrist (2.51).

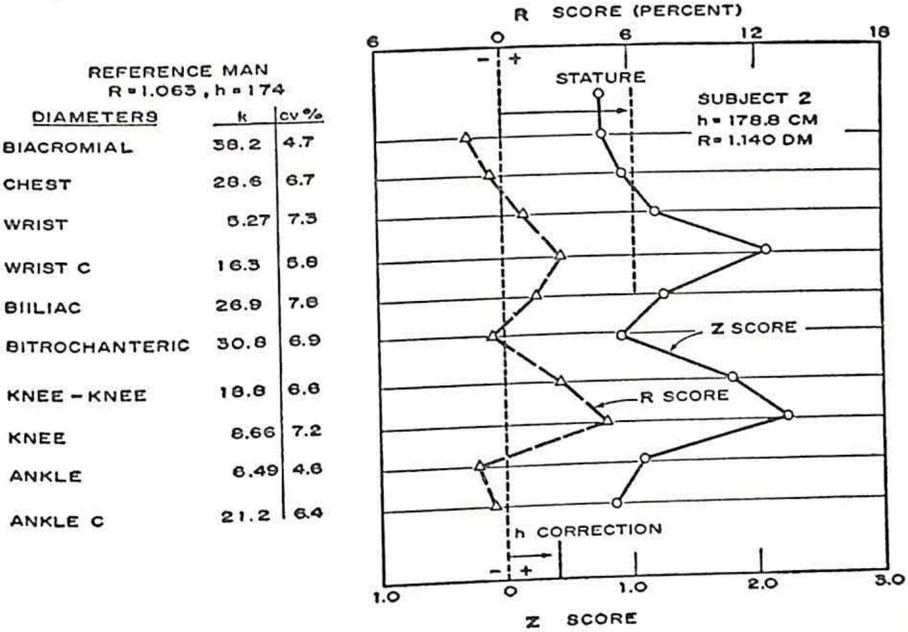

Fig. 3. "Skeletal" Profile of a Subject in Terms of the "Z" Score $\frac{X - \bar{M}}{\sigma}$ and the "R" Score [% Deviation from the Individual's Lean Body Radius (R(LBW))]. The Stature (h) Correction is Referable to the Difference Between the Subject's Stature (178.8 cm) and the Reference Stature (174 cm).

Graphical representation of lean body size.

The intra-individual variations in the skeletal measurements may be presented diagrammatically in terms of a "Z" score (Garn, 1955), or an "R" score, figure 3. The "Z" score for each dimension is computed from $Z = \frac{X - \bar{M}}{\sigma}$. It is thus the ratio of the distance from the group mean ($\bar{M}$) of the individual measurement (X), to the standard deviation (σ) of the group "X" values. The correction for stature, h (fig. 3) in connection with the "Z" score is required to adjust for the difference between the $\bar{M}$ values for the Reference Man ($h = 174$ cm) and those of the subject ($h = 178.8$ cm). In this case, the h correction is subtracted from "Z" scores of the figure.

The "R" score is the percentage deviation of any single dimension R from the individual's LBM radius (R(LBW)). If a rounded number of 6% is used as the CV for all dimensions except stature, then the "R" score is a convenient measure of intra-individual variation. For a comparison of the individual's position with respect to the group, it would be necessary to compare individual R(LBW), (for subject 2, 1.140) with group $\bar{R}$(LBW) adjusted for stature.

REFERENCE MAN AND WOMAN

Mean group values.

The mean values (table 1) of the "skeletal" measurements on three groups of men are in good agreement. Attention is invited particularly to the "skeletal" dimensions of Willoughby's muscular athletes which are similar to those of the other two groups. This was not true with respect to the girth dimensions, particularly of the upper extremities which reflected the muscular hypertrophy characteristic of weight lifters (Behnke *et al.*, 1959).

From density and body water analyses over a period of 15 years (Behnke, 1953), it has been possible to derive a relationship between stature and mean or median lean body weight for groups of men arranged according to stature, *i.e.*, mean $\text{LBW} = 0.204h^2$ (dm). Strictly, the squared value of stature should predict a median (not a mean) weight value, but the difference between mean and median values with respect to lean body weight will not make any appreciable difference in the calculations. A mean value of ~ 63 kg is obtained as the calculated value for several groups of men whose mean stature approximates 17.5 dm. The mean stature of the cadavers from which the skeletons were obtained in Trotter's study was 17.15 dm, and the calculated LBW derived from this stature is 60 kg (table 3).

With reference to women, there are no experimental data for fat-free or lean body weights comparable to those for men. If, however, the Skel I conversion constant for men, *i.e.*, $k = \sim 181$, is also applicable to women (as were the "envelope" constants relating circumferential dimensions to body weight), then it is possible to calculate $\bar{R}$(LBW) for women. Thus, for Willoughby's female group (mean stature, 16.27 dm, "skeletal" $\Sigma D = 175.5$ cm), the quotient $175.5 \div 181$ is

0.97 dm. This is an approximation of $\bar{R}$(LBW) applicable to this group of women. Likewise, from Turner's data a value of 0.96 dm ($\bar{R}$(LBW)) was obtained for a group of college girls whose mean stature was 16.45 dm. From πR^2h, their median LBW is 47.6 kg. From the values of lean body weights calculated for the several groups, it was possible to derive the mean body radius for each group and, in turn, to compute the "k" values from the mean group dimensional data. The results were so consistent that it appeared justified at this time to compute constants and dimensions of a Reference Man and Woman, table 6.

TABLE 6

"Skeletal" measurements and the dimensional (lean body weight) proportionality constants (k) for a Reference Man and Woman

DIMENSIONS D (cm)	REFERENCE MAN $R = 1.063$ dm* D	REFERENCE MAN $R = 1.063$ dm* $k = (D \div R)$	REFERENCE WOMAN $R = 0.970$ dm** D	REFERENCE WOMAN $R = 0.970$ dm** $k = (D \div R)$
Diameters				
Bitroch.	32.7	30.8	32.0	33.0
Biacrom.	40.6	38.2	35.0	36.1
Chest	30.4	28.6	25.5	26.3
Knee-to-knee	20.0	18.8	18.6	19.2
Bi-iliac	28.6	26.9	28.6	29.5
Circumferences				
Wrist	17.3	16.3	15.2	15.7
Ankle	22.5	21.2	20.6	21.2
Σ	192.1	180.8	Σ 175.5	180.8
GROUP DATA				
Age	20 to 25		18 to 25	
Stature (dm)	17.40 (68.5 in)		16.38 (64.5 in)	
Weight (kg)				
median	69.3 (152.5 lb)		56.4 (124 lb)	
mean	70.0 (154.0 lb)		56.8 (125 lb)	

* "Radius" of LBM

$\pi R^2h = 61.8$ (median LBW) $\simeq 0.204\,h^2$

** "Radius" of LBM

$\pi R^2h = 48.4$ (median LBW) $\simeq 0.18\,h^2$

Complementary relationships.

The employment of the identical conversion constant (k) for men and women implies that there exists also for the "skeletal" dimensions the same complementary relationships postulated for the girth measurements (Behnke *et al.*, 1959). With respect to the following combinations of anthropometric diameters at least, the "k" values based on the various group data referred to are equal:

$$\overset{\text{Men}}{\frac{k(1)\ \left\{\begin{array}{l}\text{Biacromial}\\ \text{Chest}\end{array}\right.}{k(2)\ \left\{\begin{array}{l}\text{Bi-iliac}\\ \text{Bitrochanteric}\end{array}\right.}} = \overset{\text{Women}}{\frac{\left\{\begin{array}{l}\text{Bi-iliac}\\ \text{Bitrochanteric}\end{array}\right.\ k(2)}{\left\{\begin{array}{l}\text{Biacromial}\\ \text{Chest}\end{array}\right.\ k(1)}}.$$

THE SKELETAL MASS AND LEAN BODY WEIGT

We may examine further the applicability of Matiegka's formula for the calculation of bone weight. In this formula, the value of the constant (k), "to be subsequently determined" was postulated to be in the range of 1 to 1.2. The median value of LBW of the cadavers from which the skeletons were obtained in Trotter's study was estimated to be ~ 60 kg. If fat-free bone weight is $\sim 12\%$ LBW, then mean bone weight $= \sim 7200$ gm. Substituting the mean values for the 8 transverse diameters measured on the disarticulated skeletons in Matiegka's formula and with k equal to 1, one obtains a calculated value for bone weight of 7547 gm.

The 8TD in the Navy group (Behnke) measured on the radiographs was 52.6 cm, and the mean stature of the 22 men, 17.77 dm. Again, substituting these values in Matiegka's formula, if $k = 1.0$, one obtains a value for bone weight of 7682 gm and

$$\text{LBW} = \frac{100}{12} \times \text{BW} = 64\ \text{kg}.$$

Gross composition of the skeleton.

The percentages by weight obtained by Forbes *et al.* (1953) in an analysis of one entire skeleton were: moisture (28), ether-extractable substance (25), and bone solids (47).

The ash weight in per cent of fat-free dry weight reported by Trotter and Peterson (1955) in an analysis of 6 complete skeletons was consistently close to 65% of fat-free dry weight. From these two sets of analyses, the relative percentages of the gross components of *fat-free bone* are: water (37), organic matter (22), the mineral substance (41). The mean value of fat-free *dry* bone for the skeletons of Trotter's series ($N = 24$) was 4460 gm. The wet bone weight, applying the above stated percentages, may be calculated as $\frac{100}{63} \times 4460 = 7080$ gm, approximately 11.8% of 60 kg, the calculated LBW.

CONCLUDING COMMENT

The constant, π, can be eliminated from the formulas in the calculation of lean body weight from dimensional values. With stature (h), and the following combination of diameters expressed in decimeters, the simplified formulas for the calculation of LBW (men) are:

Anthropometric Diameters

1. Chest, bitrochanteric, knees, ankles, wrists
   LBW (gm) $= 31.53D^2h$
2. Biacromial, chest, bitrochanteric, knees, ankles, wrists
   LBW (gm) $= 16.52D^2h$

X-ray

3. 8 transverse diameters (fig. 1)
   LBW (kg) $= 0.123D^2h$
4. 8*TD* plus chest, bitrochanteric diameters
   LBW (kg) $= 0.0287D^2h$.

Hechter (1959) has made an independent analysis of the data reported in this paper. As few as three dimensions (*e.g.*, wrist, chest, and stature) can be employed to estimate LBW. For a more complete description of the body, however, and to outline body configuration, the additional measurements are required, even though they contribute little to the accuracy of the LBW calculation.

The ratio of mean LBW for men and women of comparable stature, *i.e.*, 17.4 dm (male) and 16.4 dm (female) cannot be too far from 4 to 3. In this connection, attention is invited to the mean value of 44.6 kg for fat-free weight of women, reported by Garn (1957). His data were based upon an analysis of measurements of fat-plus-skin thickness as revealed by soft-tissue x-rays.

Another approach to the problem is the calculation of lean body weight from mean values of basal metabolism of men and women (Behnke, 1953). If 73.7 Calories per hour (1770 Cal/24 hr) is the basal heat production of young men whose $\bar{M}$ LBW is 63 kg, then the heat production per kg of LBW is 1.17 Cal per hour. Mean basal heat production values for young women have been recorded as 56.3 Cal per hour (1350 Cal/24 hr). If the metabolism per unit of lean tissue is the same in men and women, then the mean lean weight calculated from metabolism is $\sim$48 kg.

SUMMARY

1. A comparison was made of lean body weight (LBW) estimated from "skeletal" (anthropometric and "x-ray") measurements and from body density and total body water determinations on 31 male subjects.

2. The correlation coefficients (r) pertaining to this comparison were in the range of 0.80 to 0.90 (S. E. of estimate, 6 to 10% of the mean LBW, 63.4 kg).

3. The higher correlations were obtained when both trunk and extremity dimensions (D) were employed in the calculations which were based on the general formulation, $LBW = D^2kh$, where h is stature.

4. Variations in the amount of subcutaneous fat appear to be responsible for the largest source of error in estimates of LBW from the anthropometric measurements.

5. The x-ray technique for measurements of joint diameters (Matiegka's 8 Transverse Diameters) is satisfactory, but these measurements alone ($r = 0.80$) are not as highly correlated with LBW as are the combined trunk and extremity diameters.

6. The translation of "skeletal" dimensions into body radii, analogous to the dimensions of a cylinder, facilitates the comparison of an individual's dimensions with group mean values.

7. Consistent measurements converted to a common denominator in terms of the body radius were compiled for both a Reference Man and a Reference Woman.

8. Complementary relationships were revealed for upper and lower trunk, and upper and lower extremity measurements of men and women.

9. With refinements and standardization of anthropometric and roentgenogram techniques, the estimate of LBW from the size of the "skeleton" apppears to be a reliable reference standard both in health and disease.

ACKNOWLEDGMENT

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AMERICAN NEGRO-WHITE DIFFERENCES IN THE THERMAL INSULATIVE ASPECTS OF BODY FAT

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STUDIES on American White men have indicated that subcutaneous body fat acts as an insulation when the total body is exposed to moderate cold stress (Baker and Daniels, 1956; LeBlanc, 1954). For subcutaneous fat to act as insulation for the body, the blood flow to the surface must be scant, otherwise, the deep body heat produced by metabolic action would be transported to the surface and lost through the skin to the cool environment. Given the phenomena of substantial sub-adiposal vaso-constriction, the total body will be insulated according to the thickness and distribution of the subcutaneous fat. Insofar as the two characteristics may vary from one race to another, it does not seem safe to assume *a priori* that the insulative characteristics of body fat found for one racial group necessarily hold for others. It is hard to imagine racial groups which are totally lacking in a vaso-constrictive response to cold stress, but it is quite possible that there exist many shadings in degree. It is also reasonable to assume that since there are such exotic fat distributions as those shown by Bushman female, there may be substantial racial difference in subcutaneous fat distribution as well as fat quantity. This assumption is in part supported by the skin fold thickness distributions shown by Newman (1956) for American Negroes and Whites.

As part of a large study on American Negro and White reactions to cold stress, an effort was made to determine whether comparable measurements of subcutaneous fat produced comparable effective insulation for these racial groups.

To rule out group differences in physiological responses due to known morphological factors, 18 Negroes and 18 Whites were matched for body weight, stature and body fat. Selection on this basis provided a non-representative population sample as it has been shown by Newman (1956) that American Negro soldiers have less fat on the average than White soldiers of the same age. The final group reported on in this paper contains only 17 Negroes because of the inability of one subject to complete the experiment.

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METHODS

Using the Harpenden caliper (Tanner and Whitehouse 1955), skinfolds in the present series were measured on the right side at:

Arm—posterior aspect midway between acromion and the olecranon process.

Chest¹—juxta nipple—with caliper in line with the anterior of the axilla.

Chest²—on the mid-axillary line at the level of the xiphoid process.

From these skin-fold measurements, body fat was estimated using the regression equation described by Pascale *et al.* (1956). The Whites had a mean body fat of 9.8% with a 1.5 standard deviation. The Negroes averaged 9.2% with a 1.5 standard deviation. Body weight was matched within one kilogram, stature within a centimeter.

The group, dressed only in shorts, was equilibrated in a neutral temperature room for one hour before being exposed to the cold. They were then wheeled into a cold room in canvas chairs where they sat for two hours. At the end of the cold exposure they were wheeled back into the neutral temperature room where they were studied for a 30 minute follow-up period. The cold room was maintained at a dry bulb temperature of 50°F (10°C), relative humidity 50%, wind 5 mph. The neutral room was 80°F (26.7°C), relative humidity 50%. All individuals were exposed to the experimental conditions twice.

Body temperatures in this study were measured by means of thermocouples. Skin temperature was measured at 11 different points and integrated into a mean weighted skin temperature. (Baker and Daniels 1956). These skin temperatures were recorded continuously during the equilibrating pre-cold period (at 26.7°C), during the cold exposure and during the post-cold rewarming period. Rectal temperatures were measured with a thermocouple mounted in a flexible catheter (Davidzick *et al.* 1953). Readings were made from a potentiometer every 15 minutes during the test periods described above.

TABLE 1

Mean weighted skin and rectal temperatures for selected time intervals

		SKIN TEMPERATURE							
		WHITES				NEGROES			
		Mean		SD		Mean		SD	
End of Equilibration Period		90.8°F	32.7°C	.9°F	.58°C	90.5°F	32.5°C	.8°F	.44°C
Time in Cold	15 min.	78.8	26.0	1.5	.83	77.7	25.4	1.3	.72
	60 min.	75.9	24.4	1.8*	1.00	74.5	23.6	1.0*	.55
	105 min.	74.6	23.7	1.8*	1.00	72.8	22.7	1.0*	.55
After 15 min. rewarming		84.5	29.2	.8	.44	83.4	28.6	.9	.50

		RECTAL TEMPERATURE							
		WHITES				NEGROES			
		Mean		SD		Mean		SD	
End of Equilibration Period		98.3°F	36.83°C	.35°F	.19°C	98.3°F	36.83°C	.31°F	.17°C
Time in Cold	15 min.	98.3	36.83	.43*	.24	98.3	36.83	.26*	.14
	60 min.	98.5	36.94	.55*	.31	98.4	36.89	.31*	.17
	105 min.	98.2	36.77	.70	.39	97.8	36.55	.50	.28
After 15 min. rewarming		97.4	36.33	.96*	.53	96.8	36.00	.58*	.32

* Racial Difference, significant at the .05 level or better.

RESULTS

For the present analysis not all readings were investigated. Instead, selected readings, when skin and rectal temperatures were recorded simultaneously, have been used. The means and standard deviations shown in table 1 are the averages of the results obtained from the two cold exposures.[1]

The mean values show some mean differences which are discussed by Iampietro *et al.* in a different article on this study (in press). However, the standard deviations also show considerable differences between the two groups. With a few exceptions, there is a significant racial difference in the variance. Because the groups were matched for the factors known to affect body temperatures the variances were compared using an F ratio technique. The fact that the means are in some places significantly different from each other makes a statistical comparison of the variances not completely accurate. However, the error involved is small and the statements concerning the significance of the differences should be approximately correct.

TABLE 2

The correlation of per cent body fat to selected rectal and mean weighted skin temperature readings

	MEAN WEIGHTED SKIN TEMPERATURE		RECTAL TEMPERATURE	
	Whites (N-18) *r*	Negroes (N-17) *r*	Whites (N-18) *r*	Negroes (N-17) *r*
End of Equilibration Period	—.50*	—.42	.16	—.08
Time in Cold 15 min.	—.56*	—.54*	.55*	.27
Time in Cold 60 min.	—.57*	—.36	.52*	.28
Time in Cold 105 min.	—.67*	—.57*	.49*	.36
After 15 min. rewarming	—.40	—.35	.47*	.60*

* *r* significant at the .05 level or better.

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[1] In another analysis of this material presented by Iampietro *et al.* only the data obtained in the second run is presented. Since no statistically significant difference between the results of the two exposures was found, data from both exposures were used for this article.

The difference in standard deviation suggested that there might also be Negro-White differences in the effect of subcutaneous fat on body temperatures during cold exposure. Table 2 shows the correlations of body fat to those temperature measurements.

The correlation coefficients of body fat to skin and rectal temperatures found in Whites are the same direction and of similar magnitude to those reported by Baker and Daniels (1956) when a different group of White men was exposed to 60°F for two hours. However the r values for the Negroes are lower and often not significant. This would appear to be partially a consequence of the lower variability in Negro temperature responses. However, the consistency of the correlation coefficients for the Negroes in relation to the comparable figures for Whites indicate that body fat also has a biologically significant relationship to Negro body temperatures in the cold.

To demonstrate the racial differences in the role of fat in temperature regulation, regression equations were derived for all the periods, with the exception of pre-cold rectal temperature and post-cold mean weighted skin temperatures (where no significant correlation was found for either group). From these regression equations, predictions at each time interval were made for 7, 9, 11 and 13% of body fat. These values were plotted to characterize the cooling curves for individuals with these differing degrees of adiposity. The results are shown in figures 1 and 2. Although only four points were evaluated for each cooling curve, these points were connected with a curve because of the known characteristics of body temperature cooling curves (Baker and Daniels 1956).

An inspection of these figures indicates that at all time intervals, variation in fat for the Whites produces greater variations in body temperatures than comparable fat variation among the Negroes. This racial difference seems to increase with length of time in the cold so that at the end of the cold exposure period there is the greatest difference in skin temperatures, and during the rewarming period there is the greatest difference in rectal temperatures. At the last reading made in the cold, a Negro with 7% body fat had a predicted skin temperature 3 degrees Fahrenheit lower than a comparable White; while a Negro with 13% fat had only a 0.8 degree F lower skin temperature. After 15 minutes of rewarming, the Negro with 7% body fat was predicted to have a rectal temperature 0.2 degree F lower than the White with comparable adiposity. However, the Negro with 13% body fat had a rectal temperature some 0.8 degree F lower.

DISCUSSION

The present study supports the conclusion that subcutaneous fat acts as insulation during cold exposure but it also indicates that comparable amounts of adiposity on Negroes do not appear to act as comparable barriers to heat loss. This might be no more than a product of only three direct measurements of skinfolds. The formula for predicting fat was derived from results obtained on Whites only, and a markedly different patterning of fat might exist for the Negro. The high intercorrelation between subcutaneous fat measurements demonstrated by Garn (1954), does not seem to support the idea of major error in this area. However, after investigating the interrelations of skinfolds for both Whites and Negroes, Newman (1956) concluded that there were patterning differences between young men of the two races, indicating that a formula calculated for Whites might not hold for Negroes. Even though the formula might not be completely valid as applied to American Negroes it is difficult to see how the errors involved could have led to

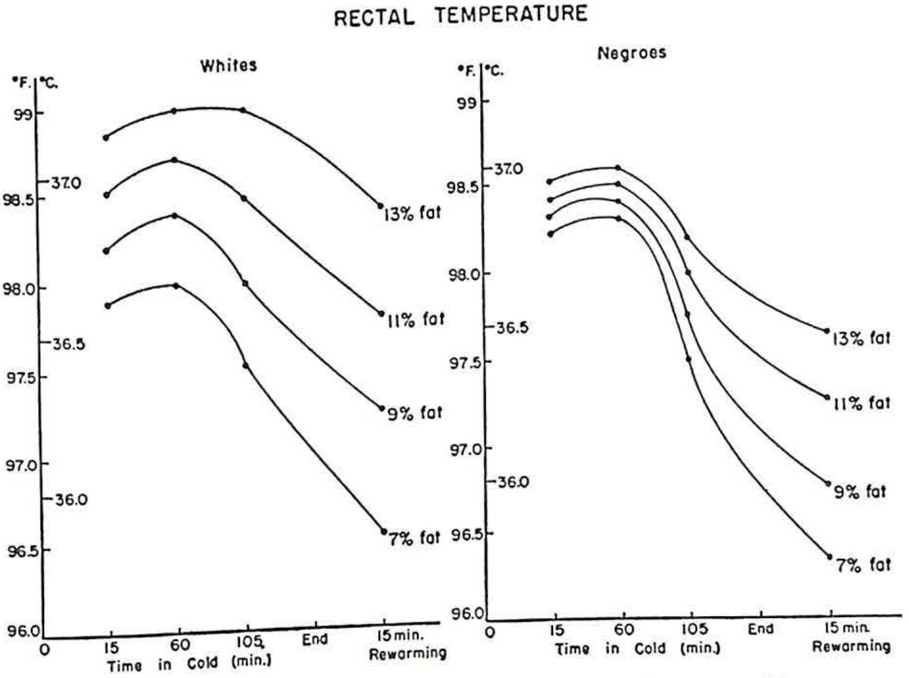

FIG. 1. RECTAL TEMPERATURE COOLING CURVES FOR AMERICAN NEGROES AND WHITES. POINTS ON THE GRAPHS ARE DERIVED FROM THE PREDICTION REGRESSION OF TEMPERATURE ON BODY FAT. AMBIENT TEMPERATURE 50°F.

the selection of a Negro group with a much more limited range in body fat than Whites. Since, in many cases the standard deviation for Negroes in body temperature was only a little over half the value for Whites, we would have to assume an error of this magnitude before the racial differences in temperature response to cold could be explained on the basis of poor subcutaneous fat estimates.

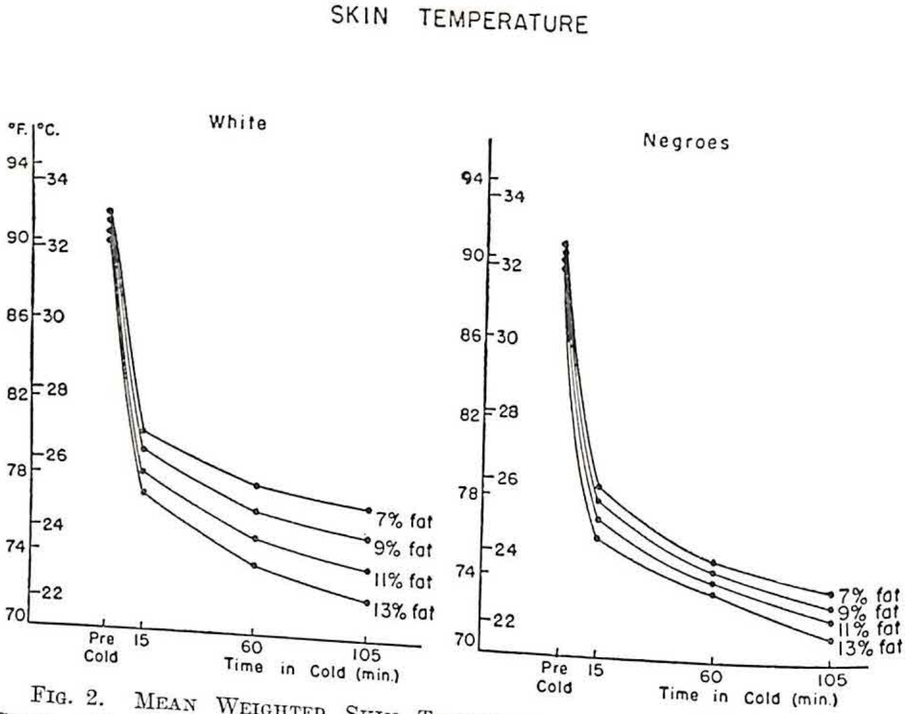

FIG. 2. MEAN WEIGHTED SKIN TEMPERATURE (11 POINT MEASUREMENT) COOLING CURVES FOR AMERICAN NEGROES AND WHITES. POINTS ON THE GRAPHS ARE DERIVED FROM THE PREDICTION REGRESSION OF TEMPERATURE ON BODY FAT. AMBIENT TEMPERATURE 50°F.

Another possible explanation is based on the fact that internal to external heat exchange in the cold is affected only by the immediate fat between the muscle and skin surfaces and is probably not affected by internal deposits of fat. Hooton (1946) and Schultz (1925) demonstrated that the American Negro has relatively longer forearms, hands, legs and feet than American Whites of the same stature. This suggests that, although the present samples of races were very closely matched for height and weight, the Negroes may have had greater surface areas, and this surface area may not have been distributed in the same proportions. If, as seems probable, the Negro has a greater percentage of his surface area in the extremities, then the findings of this study could be explained by the fact that subcutaneous fat is concentrated centrally and greater quantities of fat would do very little to reduce heat loss in the forearm, leg, hand and foot. This would also explain the lower correlation coefficients for Negroes between body fat and body temperatures since, again, variations in fat would have scant effects on far extremity heat losses.

There remains also a possible racial difference in the degree of sub-adiposal vaso-constriction. If such a difference existed, it would also produce the racial differences in body temperature variation noted in this study. A study in which the subjects were carefully matched for local distribution as well as total body fat should help in choosing between the proposed alternative explanations.

CONCLUSIONS

Irrespective of the merit of the above speculations, three conclusions may be derived from the data.

First: Body temperatures of the American Negroes in this study showed significantly less variation than the American Whites, even though the groups were matched for the known body morphology factors which affect these temperatures.

Second: Significant correlation between adiposity and body temperatures were found for both Negroes and Whites. Since the American Negro soldier has substantially less subcutaneous fat than the American White soldier, representative samples would have shown the Negro-White rectal temperature difference greater than reported in this study when both were placed in these experimental cold conditions.

Third: Although significant correlation was found, the majority of body temperature variation was not accounted for by subcutaneous fat; nor can it be explained by metabolic variations since the means and standard deviations for oxygen consumption on the same subjects were found by Iampietro (in press) not to be significantly different.

ACKNOWLEDGMENTS

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SECULAR CHANGE IN THE INTELLIGENCE OF 11-YEAR-OLD ABERDEEN SCHOOLCHILDREN

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IN the course of their review of secular change in height and weight of British children, Boyne, Aitken and Leitch (1957) found evidence from data obtained between 1914 and 1953 that girls were overtaking boys in weight at an earlier age in the latter half of this period than in the first part. From the data for 14-year-old children for the years 1948 to 1953, they detected a highly significant trend in both height and weight in that the boys were taller than the girls by a progressively increasing amount [$P < 0.001$], and the weight advantage which girls held over boys became progressively less, between these years [$P < 0.02$]. Since Bayley (1956) and other writers have pointed out that the adolescent spurt in girls occurs two years earlier than in boys, these results suggested that the physical growth spurt associated with puberty was starting earlier year by year.

Although British records do not go far enough back, Tanner (1958) has quoted Norwegian statistics to show that the average age of a girl's first menstruation has fallen from 17 years in 1850 to 13¾ years today. Presumably there has been a corresponding shift in the age at which puberty occurs in boys. Cuthbertson (1958) in his review on the adolescent quotes several workers who have demonstrated that maximal height is becoming greater and is being reached progressively earlier.

In this paper the authors examine the results of intelligence tests carried out in Aberdeen on 11-year-old children, to see whether these suggest any secular change in intelligence corresponding to the physical changes outlined above.

OTHER INVESTIGATIONS

The two principal investigations into changes in intelligence in Great Britain have been those of Emmett (1950) and the Scottish Council for Research in Education (1949). Emmett in 1950 reported the results of his investigations into the trend of intelligence in certain parts of England. He included only those districts where the children examined before and after the war were considered to be comparable. In 1947 in each educational authority district the children were submitted to a Moray House Intelligence Test[1] for 11-year-olds which had been carried out, on average, some 9½ years earlier in that area. Wherever possible, the results for boys and girls were examined separately when appropriate adjustments had been made for differences in the age at which the early and late groups of children had been tested. The results were interpreted on the basis of the actual scores made in the tests and on the derived intelligence quotients (I. Q.). The author's main conclusion was that a highly significant fall in the I. Q. of boys and an equal rise in the I. Q. of girls had taken place over the period.

Commenting on this result, Emmett stated that on the whole the conditions under which the two tests were made and the composition of the two groups were as nearly comparable as it was possible to make them. It was his opinion that changes in schooling probably caused the scores in the second test to be depressed, although it was impossible to determine the extent of this effect.

In the Scottish study no sex difference in I. Q. was found in the pre-war group of children, but in the post-war group there was a significant difference in favor of the girls.

Whatever interpretation be placed upon changes in absolute values over the war years, there is little doubt that at the time of the 11+ examination the intelligence of girls, as measured by the test, had improved relative to that of boys over this period.

In three of the districts in which girls in 1947 had a higher I. Q. than boys at 11+ years, Emmett later carried out a test when the children were 13+ years old. He found that the superiority of girls over boys fell significantly between these two ages and concluded that the general post-war superiority of girls at 11+ was transitory, attributing the sex difference to some change in environment since the pre-war years which had a different effect on each sex.

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[1] The Moray House intelligence tests are verbal, objective, timed, group tests of intelligence which are prepared every year by the Education Department of the University of Edinburgh and are published by the University of London Press for restricted sale to Education Authorities throughout Great Britain.

Secular change in mental age among certain Canadian schoolchildren was examined by Binning (1958). He compared the I. Q.'s of Saskatoon schoolchildren in 1946-7 and 1955-6, and found that an increase had taken place over this period. The change in mental age was accompanied by faster growth in height and weight, by an earlier loss of visual acuity, and probably also by an earlier onset of puberty. Binning, however, did not make a sex comparison, nor did he specify the age of the children, but from the context this appears to have been about 12-13 years.

In these three investigations quoted, an attempt was made to assess changes in intelligence by comparing the test-passing ability of children on two occasions separated by a number of years. The two British investigations suffer the handicap that the later groups of children were schooled during the war years with all their associated disruptive effects. Emmett was aware of this, and considered that this depressed the absolute scores obtained on the second occasion. But Tanner (1958), in an address to the Oxford Institute of Education, considered that it was impossible to tell whether there had been advances in mental age over the years, since the advance of maturation meant that comparing the test-passing ability of a ten-year-old now and that of one 30 years ago would not be comparing like with like. To this we would add that over such an extended period there must also have occurred a great change in the way of life, another relevant factor.

THE PRESENT INVESTIGATION

In view of the difficulties of interpreting changes in I. Q. mentioned above, it was decided not to attempt this, but to pursue further the sex differences encountered by Emmett and the Scottish Council.

Compared with many other parts of the country, Aberdeen possesses a stable population, in that the community is very self-contained, and there is little population movement between it and other regions. Complete records have been kept by the Department of Education for the City of the results of 11+ examinations from 1946 to the present date, and an examination was made of the data pertaining to 1947 and 1957. 1946 was not selected, lest there might have been some abnormality in the results of the tests during this first post-war year which was also the first year in which a test had been carried out since before the war. The year 1957 was the latest available at the time of the enquiry.

In each year two Moray House Intelligence Tests were applied to the children by their teachers. The results of the first of these were analysed, after they had been adjusted for each child to an age of 11 years. Each year approximately 3000 children were tested. The results in table 1 show that the I. Q. of the girls relative to that of the boys had risen between 1947 and 1957 by 1.7 points [$P < 0.02$]. As there might, by chance, have been a change in the test so that the later one favored girls more than the first had done, the results from an intermediate group were examined. For reasons unrelated to this enquiry it was most convenient to use the data from 1951. When the average superiority of boys over girls is plotted against year of examination, as in figure 1, the three points lie very nearly on a straight line. The change, whatever its cause, seems to be gradual, and has been taking place since pre-war years.

TABLE 1

Mean I. Q. of boys and girls separately in 1947 and 1957

	BOYS	GIRLS	DIFFERENCE (B-G)
1947	102.1	101.3	0.8
1957	104.6	105.5	—0.9

Improvement of girls relative to boys between 1947 and 1957 = 1.7 ± 0.79.

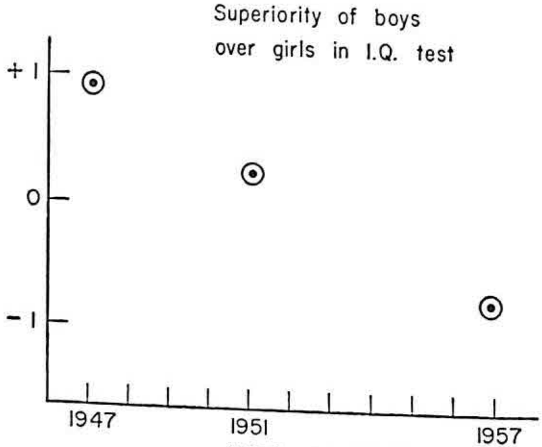

FIG. 1. SECULAR CHANGE IN RELATIVE INTELLIGENCE OF BOYS AND GIRLS AS MEASURED BY MEAN DIFFERENCE IN SCORE (ADJUSTED FOR AGE) IN 11+ MORAY HOUSE INTELLIGENCE TEST.

These Aberdeen data were examined further. Table 2 shows the age distribution of the children tested on each occasion. It is similar for boys and girls, but the age distribution in 1947 is very different from those in 1951 and 1957. Such differences as exist between the age distributions in 1951 and 1957 are probably due to the effects on birth rate of the onset and ending of the war. There is a greater proportion of older children in 1947, probably a carry-over from war-time conditions in schools. The average age at test is 11 years 10 months in 1947, 11 years 7 months in 1951 and 11 years 6 months in 1957.

TABLE 2

Age distributions by sex of children tested in 1947, 1948, 1951 and 1957

	1947		1948		1951		1957	
AGE IN MONTHS	B	G	B	G	B	G	B	G
–126	3	2	1	0	1	0	0	0
126–129	7	3	7	3	3	0	2	1
129–132	22	22	24	34	36	27	19	20
132–135	117	111	131	110	139	134	215	207
135–138	201	224	305	282	274	262	381	357
138–141	219	285	313	327	313	279	344	304
141–144	288	278	318	312	290	280	272	232
144–147	216	224	215	185	158	164	142	141
147–150	136	118	113	112	46	33	18	21
150–153	107	82	93	69	1	9	0	1
153–156	26	23	25	19	5	5	0	1
156–159	37	36	28	18	4	3	0	0

Most children tested were between 11 years and 12 years 3 months old at the time of the test in 1951 and 1957. The children outside this age range had been promoted or retarded from the classes tested in the subsequent or previous years respectively, and by analogy few children outside this age range had been promoted or retarded to the classes tested in 1950, 1952, 1956 or 1958. The intelligence quotients and sex differences are given in table 3 for this control block of children at three-monthly intervals, and also for the corresponding groups of children in 1947, although the 1947 children may not be strictly comparable with the other groups, as a result of the greater numbers who had been retarded into the 1948 group. This is shown in table 2 where the age distribution of children tested in 1948 is also given, and those over 12 years 3 months of age were almost all retarded from the 1947 group.

There were irregularities, as would be expected with such small numbers, but the tendency was for the shift in favor of the girls to be present at all ages. This age range, from 11 years to 12 years 3 months, has also manifested a similar shift in favor of girls with respect to physical measurements (Cuthbertson, 1958) as a result of the earlier timing of the adolescent spurt in girls. The effect of the earlier spurt in boys, as manifested by the change in age at which boys overtake girls physically, does not show itself until about 14 years of age (Boyne *et al.*, 1957). Analogous to this there were the results of Emmett's examination of the intelligence in 1949 of 13-year-old children who had been examined at 11+ years in 1947. These showed that the superiority which the girls had achieved at 11+ years was transitory and the superiority of the boys was re-established by the time of the second examination. Emmett regarded the superiority of the 11+ girls in 1947 as a result of environmental change and he suggested that the effect of the war years was more detrimental to the boys than the girls. But in the years following the war, with the re-establishment of peace-time conditions and the return of fathers to disrupted families, the same intelligence trend continued at 11 years of age.

TABLE 3

Mean I. Q. at 3-monthly intervals of central group of children tested in 1947, 1951 and 1957[1]

AGE IN MONTHS	1947			1951			1957		
	B	G	B-G	B	G	B-G	B	G	B-G
132–135	111.3	107.6	3.7	109.9	110.0	—0.1	109.0	110.5	—1.5
135–138	108.4	107.4	1.0	104.2	104.0	0.2	104.0	105.1	—1.1
138–141	107.7	104.1	3.6	101.7	101.6	0.1	104.6	105.2	—0.6
141–144	102.7	102.8	—0.1	100.1	100.4	—0.3	104.2	103.8	0.4
144–147	99.7	98.8	0.9	98.6	98.0	0.6	99.8	103.7	—3.9

[1] It is of interest that the I. Q. of the youngest children tends to be high, that of the oldest children low. This does not reflect unfavorably on the age correction, but arises because the first group contains a leavening of children promoted from the following class at some stage in their school career, and the last group contains a number of children retarded from the previous class.

DISCUSSION

If, as reported by Emmett, the fall in intelligence of the boys over the war years was real, then the situation is complicated and difficult to explain, but Emmett himself, as reported above, and Tanner (1958), have commented on the difficulties of comparing absolute values of intelligence in different years. Otherwise, these results here reported are entirely analogous to the accepted position in physical growth, where the female adolescent spurt precedes that of the males by about two years and both are occurring earlier year by year.

Hypothetical intelligence curve

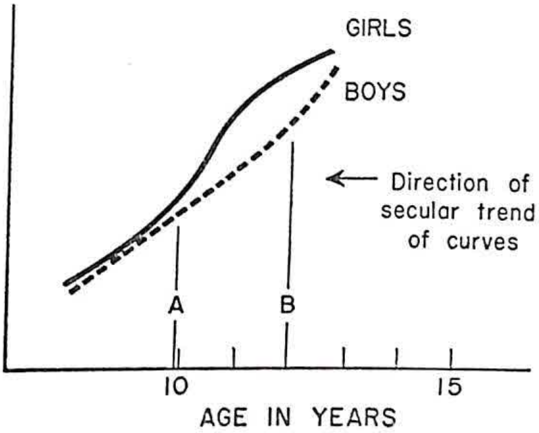

Fig. 2. Hypothetical Intelligence-Age Curves to Illustrate the Suggested Effect of an Advancing Adolescent Spurt upon the Relative Intelligence of Boys and Girls.

Figure 2 suggests a reason for the change in I. Q. difference between the sexes. It shows hypothetical curves of intelligence plotted against age. These are similar in shape to weight curves and it is suggested that growth in intelligence may follow a similar pattern to weight increase, *i. e.*, in each sex there is an acceleration in growth rate associated with adolescence, and over the years this spurt has gradually occurred at an earlier age in the child. At the time to which the diagram refers the onset, A, of the girls' adolescent spurt in intelligence takes place at about ten years of age, that of the boys, B, about 12. A comparison at age 11 years would show the girls to be slightly more intelligent than the boys. Some years earlier the curves would have been displaced to the right with respect to the age scale and a comparison at age 11 years would have shown little or no sex difference. Some years later a comparison at the same age would show a greater difference in favor of the girls as the curves would be displaced to the left relative to the age scale.

It would be dangerous to force the analogy further, and suggest that, as in physical growth, a greater intelligence is being attained with the passage of the years. Indeed, there may well be other explanations for the phenomena reported here, but the one put forward is consistent with changes which have taken place in physical growth, and it would be of great interest to know whether data from other centers confirm the trend described in this paper.

The authors wish to express their indebtedness to Dr. J. D. Nisbet, of the Education Department of the University of Aberdeen, and to Dr. D. P. Cuthbertson, Director of the Rowett Research Institute, for stimulating discussions relevant to this investigation.

SUMMARY

Work has been reported, pointing out that the adolescent physical growth spurt, which occurs about two years sooner in girls than in boys, has been occurring progressively earlier in Great Britain and elsewhere. The maximal height attained has also become greater over the years.

This investigation provides data relevant to secular change in intelligence with particular reference to the difference between the sexes. It is concluded, after reference to earlier work and an examination of fresh data obtained from the City of Aberdeen Education Department, that the intelligence of girls at 11+ years of age, as measured by the Moray House Intelligence Test, has been improving relative to that of boys from before 1939 until the present day.

From the Aberdeen data examined in 1947, 1951 and 1957, it appears that the change in relative intelligence occurs in all the children between 11 years and 12 years 3 months of age. One worker showed that the superiority of girls at 11 years in 1947 was significantly reduced at 13 years of age in 1949. It is suggested that a secular advance of the adolescent spurt in mental growth, parallelling the advance in the physical growth spurt, offers a satisfactory explanation of the results reported in this paper.

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ARM STRENGTH AND BODY DIMENSIONS

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THERE has been a number of investigations (reviewed in Cureton, 1947; Hunsicker and Greey, 1957) of the relationship between strength and physique. Claims for their positive association in adults have been based primarily on studies in which strength has been assessed by some composite index, derived from a battery of dynamometer and performance tests, and physique has been rated in terms of endomorphy, mesomorphy and ectomorphy, though actual values of the correlation coefficients have not been high. Morphology assessed metrically, however, for example by the aid of limb girths or volume, has shown both in men and women such low correlations with strength indices that Cureton was led to report (*op. cit.*, p. 362) that the size of arms and legs fails to indicate their strength, while overall measures of body size, height and weight singly and together, also have given low correlations with strength indices. On the other hand when the strengths of specific muscular actions are considered, particular anthropometric measurements are seen to assume some importance. For example, forearm girth corrected for fat was shown by Carpenter (1938) to be significantly correlated with grip; Clarke (1954) demonstrated the importance of upper arm girth (flexed-tensed) in the strength of shoulder movements and of elbow flexion, a lesser effect of upper arm length, and some effect of stature; Elbel (1949) showed a high correlation of leg strength with body weight. In children, the association of body size and grip strength has long been known (Metheny, 1941). In an endeavour to clarify this somewhat unsatisfactory situation it was thought worthwhile to examine the degree of association between arm strength, arm morphology in terms of length and girth measurements, and body size. Moreover the problem is of theoretical interest in view of the attention recently given (Coon, Garn and Birdsell, 1950; Roberts, 1953a, b) to the adaptive significance of limb morphology, for strength must have some selective role. The results may also be of practical assistance in questions of personnel selection and human engineering, where it may be necessary to consider the actual forces that operators of a particular range of body sizes are able to exert. The work is thus a development from a series of previous investigations (Provins, 1955a, b; Provins and Salter, 1955) of the effect of limb position and other variables on arm strength, in which relative values were established for the various conditions tested.

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[1] Medical Research Council Unit for Research on Climate and Working Efficiency.

MATERIAL AND METHODS

The subjects were young adult male Royal Naval personnel. The following seven anthropometric measurements were taken by a single observer on the nude subject:

Stature—Height to vertex, with subject standing comfortably erect, but not in the forced attitude of attention, looking horizontally before him.

Weight—Subject standing on a platform spring balance.

Upper arm length—From acromion to upper margin of head of radius, the arm hanging straight with muscles relaxed. (Right arm).

Forearm length—From upper margin of head of radius to tip of styloid process of radius. (Right arm).

Hand length—From tip of styloid process of radius to tip of middle finger, the hand being placed flat on a bench. (Right hand).

Upper arm girth—At a point midway between acromion and point of elbow, the arm hanging loosely with muscles relaxed. (Right arm).

Forearm girth—Maximum girth, the arm hanging loosely with muscles relaxed. (Right arm).

Three tests of muscle strength were administered:

Hand grip strength—Using a Salter's improved hand grip dynamometer, the subject was encouraged to exert his maximum effort; the better of two trials was recorded.

Elbow flexion strength and *elbow extension strength*—Using a strain-gauge dynamometer as described by Provins and Salter (1955), the subject was instructed to apply a steadily increasing force to the handle, attaining a maximum after about five seconds; the peak galvanometer reading was recorded. The elbow was flexed to a right angle, and the dynamometer handle was vertical so that the subject's hand was approximately midway between pronation and supination.

The strength of grip was measured on both the right and left sides; as there was no significant difference between the means ($t = 1.507$), the value for the right hand only has been used in the present analysis. The tests of strength of elbow flexion and extension were carried out on the right side in 60 of the 75 subjects examined and on the left in the remaining 15; choice of side was determined by convenience of setting up the apparatus. In view of the relatively slight differences in performance between the two sides previously noted in this task (Provins, 1955a), the inconsistency in side used was disregarded in the present study.

TABLE 1

Means and standard deviations of measurements in the whole sample

MEASUREMENT	MEAN	S. D.	NO. IN SAMPLE
Stature	173.2 cm	6.47 cm	63
Weight	66.6 kg	6.41 kg	63
Upper arm length	32.9 cm	2.21 cm	60
Forearm length	25.7 cm	1.32 cm	60
Hand length	19.6 cm	0.83 cm	59
Upper arm girth	27.8 cm	2.04 cm	62
Forearm girth	26.7 cm	1.30 cm	62
Hand grip strength	41.6 kg	5.89 kg	72
Elbow flexion strength	5.90 kg/m	0.85 kg/m	75
Elbow extension strength	5.03 kg/m	0.89 kg/m	75

RESULTS

The mean, standard deviation, and number of subjects measured, for each of the anthropometric measurements and tests of strength employed is given in table 1. It was unfortunately not possible to record all the measurements on every subject, so that the number in the sample for each item varies somewhat. However, all the measurements listed were recorded for 41 men and in consequence the following analysis has been carried out on the data from this smaller series.

The simple correlation coefficients between the various measures of strength and anthropometry are set out in table 2. It will be seen that all the values for r are positive except those between upper arm girth and the four longitudinal measurements taken (viz. stature, upper arm length, forearm length and hand length); none of these four however is statistically significant. Forearm girth shows low positive correlations with the four longitudinal dimensions although the value for r is significant only in the case of hand length. As expected, the measures of girth are quite highly correlated with one another and with body weight, while there is a similar good correlation between the various measures of length.

TABLE 2

Correlation Coefficients

MEASUREMENTS	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)	(i)
(a) Stature	—								
(b) Weight	0.48	—							
(c) Upper arm length	0.75	0.49	—						
(d) Forearm length	0.68	0.25	0.35	—					
(e) Hand length	0.73	0.46	0.51	0.77	—				
(f) Upper arm girth	—0.19	0.56	—0.10	—0.06	—0.02	—			
(g) Forearm girth	0.26	0.74	0.16	0.22	0.39	0.70	—		
(h) Hand grip strength	0.48	0.42	0.41	0.41	0.47	0.19	0.38	—	
(i) Elbow flexion strength	0.45	0.47	0.34	0.47	0.59	0.40	0.64	0.56	—
(j) Elbow extension strength	0.44	0.68	0.41	0.21	0.42	0.44	0.70	0.31	0.63

Significance levels: 5%, $r = .30$
1%, $r = .39$
0.1%, $r = .49$

The measures of elbow flexion and extension strength are quite highly correlated with one another; they are significantly correlated with grip strength and with the various anthropometric measurements except in the case of elbow extension strength and forearm length. Hand grip strength however tends to be less closely correlated with the girths and weight than are the other measurements of strength.

Table 3 shows the multiple correlation coefficients of the strength measurements with each of the other dimensions when stature and weight are included. The values for R are slightly but consistently lower for flexion than for extension, and for grip than for the elbow strengths. By comparison with the first order correlation coefficients, when body size is included there is an appreciable increase in the fraction of the total variance—(a) of flexion accounted for by each of the other measurements except extension and hand length, (b) of extension accounted for by each except forearm girth, (c) of grip strength accounted for by each except flexion and hand length.

That the dependence of strength on the limb measurements is not due entirely to their mutual association with body size is confirmed by the

TABLE 3

Multiple correlation coefficients

	GRIP STRENGTH	ELBOW FLEXION STRENGTH	ELBOW EXTENSION STRENGTH
Upper arm length, stature and weight	0.52	0.54	0.69
Forearm length, " " "	0.54	0.59	0.70
Hand length, " " "	0.55	0.63	0.70
Upper arm girth, " " "	0.56	0.69	0.72
Forearm girth, " " "	0.55	0.73	0.76
Hand grip strength, " " "	—	0.63	0.69
Elbow flexion strength, stature and weight	0.62	—	0.77
Elbow extension strength, " " "	0.52	0.66	—

TABLE 4

Standard partial regression coefficients

		GRIP STRENGTH	ELBOW FLEXION STRENGTH	ELBOW EXTENSION STRENGTH
Upper arm length	excluding stature and weight	+0.05	—0.12	0.00
Forearm length	" " " "	+0.19	+0.35	—0.09
Hand length	" " " "	+0.25	+0.50	+0.08
Upper arm girth	" " " "	+0.29	+0.66	+0.30
Forearm girth	" " " "	+0.24	+0.74	+0.48
Hand grip strength	" " " "	—	+0.38	—0.02
Elbow flexion strength	" " " "	+0.39	—	+0.39
Elbow extension strength	" " " "	—0.03	+0.53	—

standard partial regression coefficients (table 4). When the effects of stature and weight are excluded, prediction of flexion strength appears to depend especially on the girths of the lower and upper arm, less markedly on the lengths of hand and forearm and not at all on upper arm length, while the other measures of strength are also important.

To a much lesser extent, prediction of extension strength also appears to depend primarily on forearm and upper arm girths but is entirely independent of the longitudinal measurements of the arm. Estimation of grip strength appears to depend on arm girths and hand length to some extent but the regression coefficients are quite low in each instance. Flexion strength appears to be very important in predicting both strength of grip and strength of elbow extension.

To examine the pattern of association further, the method of factor analysis described by Burt (1940, 1944, 1947) was employed. The saturation values for each factor derived by Hotelling's method of principal components are set out in table 5. It will be seen that all the variables show a high factor saturation for the first factor (except perhaps upper arm girth). The second, bipolar factor shows positive saturation values for the circumferential, weight, and strength measures (except grip) but negative saturations for all four measurements of length. The third factor appears to be positively associated with strength of elbow flexion and grip, the arm girth measurements and forearm and hand length, but negatively associated with height, weight, strength of elbow extension and upper arm length. The first factor accounts for 52.9% of the total variance and the second and third factors for 21.5% and 9.3% respectively. The interpretation of these results will be considered in relation to other methods of analysis in the discussion.

TABLE 5

Factor saturations

	FACTOR I	FACTOR II	FACTOR III
(a) Stature	.757	—.545	—.179
(b) Weight	.790	.329	—.316
(c) Upper arm length	.635	—.413	—.550
(d) Forearm length	.628	—.495	.444
(e) Hand length	.786	—.395	.216
(f) Upper arm girth	.376	.836	.117
(g) Forearm girth	.730	.574	.076
(h) Hand grip strength	.661	—.099	.205
(i) Elbow flexion strength	.801	.146	.307
(j) Elbow extension strength	.758	.329	—.256

DISCUSSION

(i) *The sample.* The heights and weights of the 63 men shown in table 1 are somewhat above the average reported by Martin for the general population of men in Britain aged 20–21 years (mean stature 171.45 cm, S. D. 6.65 cm and mean weight 61.55 kg, S. D. 7.50 kg). Part of this difference is probably due to sampling error, as larger samples of the height of Royal Naval personnel (Geoghegan, 1953; Geoghegan, Roberts and Willson, 1957) give values slightly lower than that shown here, but weight somewhat higher. It may be partly due to a slight age effect, since the average age of the present sample was 21.9, or to the observed secular trends, since Martin's figures were obtained in 1939, while the present work was undertaken in 1953. Other contributory explanations may lie in the medical selection of enlisted personnel since Martin's data include those rejected for service, or the regional basis of naval recruiting referred to by Geoghegan (1953), for in the present small sample, 40 of the 63 men examined were drawn from parts of Britain recording a higher mean value for both heights and weights than the national average (Martin, 1949).

The results suggest that motivation was good. The values for grip strength recorded in the present survey are lower than might have been expected from other investigations (Jones, 1949; Burke, Tuttle, Thompson, Janney and Weber, 1953) although the absolute values recorded are notoriously dependent on the instrument used (Metheny, 1941). However, the results of the tests of elbow flexion and extension strength are directly comparable with the values previously recorded with the same instrument in the studies by Provins (1955) and Provins and Salter (1955). These investigations included both male and female subjects, but the results for male subjects alone agree closely with the present findings for flexion and are slightly lower than the present values for extension.

(ii) *The findings.* The following interpretation of results should only be regarded as tentative, for the present work is based on a small number of subjects (41) and in factorial studies the type of result obtained is dependent not only on the form of analysis employed (Thurstone, 1946, 1947) but also on the number and range of the original measurements (Howells, 1951). In essence, however, the findings are closely in accord with those implicit in Clarke's (1957) study of the relationship between various anthropometric measures and trunk and lower limb strengths.

First, the fact that the anthropometric and strength values are in general positively correlated with one another suggests that, on the whole, the person who tends to be large in one body dimension tends to be above average in all body measurements as well as in limb strength. This suggestion is confirmed by the high first factor saturation (table 5) for all variables and not only agrees with but extends previous evidence for a common size factor (Burt 1938, 1944; Cohen 1938, 1941; Hammond 1942; Rees and Eysenck 1945; Burt and Banks 1947; Howells 1951) to include limb strength.

Secondly, the better correlation of the longitudinal measurements with one another than with the girth measurements which also correlate well with one another and with weight is suggestive of some fundamental distinction between longitudinal and circumferential aspects of body size. This is further demonstrated by factor II, in agreement with the findings from previous factorial studies, using different body dimensions. However, while the measures of strength of elbow flexion and extension are positively loaded in factor II as are the measures of girth and weight, strength of grip by its negative saturation appears to be associated more closely with the measures of length. For this the differences in technique of measurement may provide a partial explanation. The strengths of elbow flexion and extension were measured as torques about the elbow joint, so that the value recorded in each instance is the product of the force exerted by the muscle at its insertion and the distance of the insertion from the fulcrum of the joint. In other words, for flexion and extension strength, the variable of lower arm length has been eliminated *in the method of measurement*. The amount by which this variable continues to affect the results may be an indication of the association between the distance of the muscle insertions from the elbow joint and the length of the arm. By contrast, in measuring the strength of grip, the dynamometer employed was not capable of being adjusted to variations in hand size to suit each subject. It seems likely therefore that the association of grip strength with the measures of length rather than girth may be due, at least in part, to the fixed grip dimension of the dynamometer.

Nevertheless, there is evidence for an association between grip strength and arm girth although less pronounced than that for the other two measures of strength. This is seen most clearly in tables 3 and 4 where the influence of stature and weight on the association between the various anthropometric measurements and tests of strength is considered. There is also confirmatory evidence from the positive factor saturation of the girth measurements and grip strength for the minor factor III in table 2.

The interpretation of factor III is difficult in view of the small proportion of the variance for which it accounts, the limited number of features considered, and the small sample size. However, as the measures of overall body size are grouped with upper arm length and elbow extension strength in contrast to the other measurements recorded, it is possible that factor III may indicate local variations in the effectiveness of the common size factor.

SUMMARY

The mutual associations of arm strength, arm morphology, and body size have been examined and three factors isolated to account for the greater part of the variance. A general size factor common to all the dimensions is found to extend also to the strength measurements. A second bipolar factor, in morphology contrasting length with girth, appears to distinguish elbow strengths from grip strength; it is suggested that this is to some extent attributable to differences in technique of measurement. The effect of body size and limb dimensions, especially the arm girths, on arm strength is clearly established.

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THE GENETIC EFFECTS OF REPRODUCTIVE COMPENSATION *

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THE simplest model of natural selection in a mendelian population consists of a succession of discrete generations each produced by a random combination of the gametes of the preceding generation. The zygotes show a Hardy-Weinberg distribution of genotype frequencies, which are modified by selection before the next generation of gametes is produced. Natural populations are more complicated than this, and population geneticists have analysed the effects of some of the possible complications, such as overlapping generations and varying degrees of inbreeding, and have thereby evaluated the associated departures from the general model. The present paper presents an evaluation of the effect of *reproductive compensation* on the selection of deleterious recessives.

Reproductive compensation may be defined as any influence of offspring mortality on subsequent reproductive functions of the parents. Such effects are readily imagined for human populations, in which family size may be influenced by custom. Dead infants or fetuses may be replaced until a socially desirable family size is attained. If the dead offspring were lost through homozygosity for a deleterious recessive, their replacement would obviously reduce the effectiveness of selection in removing early-acting deleterious genes from a population. Compensation might also work through ways other than custom and in populations other than human. The early death of a human embryo results in the rapid return of the mother to a physiological state in which a replacement might be produced. So even without the social pressures, compensation would operate by increasing the time available for zygote production. This possibility was recognized by Foy, *et al.* (1954). In viviparous animals that give birth to more than one young at a time, the death of one embryo might well mean better nourishment for the others. The number of zygotes would not be affected, but compensation would operate by raising the survival prospects for the individuals not eliminated. A formally analogous situation might be recognized in seed plants. In birds, members of smaller than average clutches have higher than average survival rates (Lack, 1954). The death of one individual would presumably result in compensation through its effect on the survival of its nest mates. Compensation is most readily credited, and is probably most important in man, but as a minor statistical effect it must be of very general occurrence.

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The first recognition of this factor is attributed to R. A. Fisher (Race, 1944). Its first critical assessment was Glass' (1950) analysis of its effect on selection at the *Rh* locus. Here there is a very special situation, the adverse selection of the heterozygous (positive) offspring of positive fathers and negative mothers. Glass' data showed that there is compensation, so that such marriages produce about the same average number of surviving offspring as other marriages. He reasoned, therefore, that there was no selection between progenies with respect to *Rh*. The only effective selection is within the progenies of negative mothers and heterozygous fathers. The homozygous negative children of such parents are more likely to survive than their heterozygous sibs. Glass concluded that selection and compensation are increasing the frequency of the negative allele in the population studied. This conclusion was later confirmed by Li's (1953) demonstration of low unstable equilibrium frequencies of the negative allele in such populations.

Selection at the *Rh* locus is an unusual problem, and its solution contributes little to the general understanding of compensation as a factor in the genetics of populations. Moreover, compensation has been invoked a number of times (*e.g.*, Bianco, *et al.*, 1952, Foy, *et al.*, 1954) as a possible explanation for unusually high frequencies of lethal or demonstrably deleterious genes that produce their adverse effects in homozygotes. Such genes often have detectable effects on the heterozygote, but in their effects on viability they can be considered recessives. Analysis of this generally significant problem of the interaction of compensation and selection for genes adversely selected in the homozygote alone (recessives) or in both homozygote and heterozygote equally (dominants) was provided by Lewontin (1953). He postulated a population in which there were two types of matings, those that were genotypically capable of producing offspring with a certain hereditary disorder (*susceptible* matings) and those that were not. His analysis indicated that selection and compensation were able, with certain reasonable rates of selection and compensation, to produce stable equilibria with the deleterious gene maintained at levels greatly in excess of those attributable to mutation pressure, and in the range often attributed to balanced polymorphism.

Lewontin's conclusion, justified by mathematical deduction, is logical on an intuitive basis as well, once his premises are accepted. At low frequencies, selection of a deleterious recessive would take place only in the progenies of matings between heterozygotes. One-fourth of the individuals in such progenies would be adversely selected. Higher gene frequencies would result in progenies in which half or all of the individuals would be of the unfavored genotype. So as the frequency of such a gene increased, not only would there be increased numbers of susceptible matings, but their average susceptibility would also increase. In Lewontin's system, however, the rate of compensation is considered constant for all types of susceptible matings. With the average effect of selection in susceptible families increasing with increasing gene frequencies, while the average effect of compensation remains the same in such families, it is easy to imagine that compensation might have a greater effect at low frequencies, that selection might be more important at higher frequencies, and that a stable equilibrium could be produced at some intermediate point.

I can conceive of no justification, however, for compensation being regarded as constant and independent of mendelian ratios in susceptible matings. Compensation has always been thought to operate through the replacement of lost offspring. This interpretation implies that there should be four times the compensation effect in a cross between individuals homozygous for a deleterious recessive as in crosses between heterozygotes. In the present paper, this assumption of direct proportionality between offspring mortality and compensation will be substituted for Lewontin's assumption of the same rate of compensation for all susceptible matings.

The usual Hardy-Weinberg model of a population provides for no consideration of feed-back from offspring to parents, such as is involved in compensation. The exact analysis of this factor might follow Lewontin's method of separate consideration of selection and compensation for all possible crosses, the summation of the appropriately modified progenies, and the dependent calculation of the combined effects of selection and compensation on the gene frequencies of the next generation. However, the assumption of compensation as a function of offspring mortality rate enormously complicates this technique. Instead I will substitute a simplified approximation that should be nearly correct for populations in which a deleterious gene (a) is present in low frequency (table 1). The simplification consists first of all of neglecting the homozygous recessives in listing the types of crosses. This procedure is justified when the recessive gene is rare because almost all of the homozygous recessives would be produced by crosses between heterozygotes (Aa). Hence, almost all of the effects of selection and compensation would take place in the progenies of heterozygote pairs. The frequency of these heterozygotes would be $2pq$ (at conception), where p is the frequency of the dominant gene, and q the frequency of the recessive. Since p is equal to $1-q$, $2pq$ would be closely approximated by $2q$, when q is very small. The frequency of AA would therefore be close to $1-2q$, and this simplification serves here as the frequency of the homozygous dominant. The greater mortality rate among the aa offspring (they have a disadvantage symbolized by s) is assumed to result in compensation to the extent c, so that the progeny of two heterozygotes is increased by the factor $1+c$.

Note that the simplifications (table 1) involve only the parental genotype frequencies. As long as there is random mating, this will have no effect on offspring genotype frequencies. The offspring will show a Hardy-Weinberg distribution and will be affected by selection in the same way regardless of the genotype frequencies of the previous generation. So no error is introduced with respect to the selection factor, $1-s$. It is only for the effect of compensation that the model is inexact and applicable only for low values of q. Also note that the model is exact even for compensation in the special case of a lethal gene. If a were lethal there would, in fact, be only two adult genotypes, and their frequencies would be exactly $2q$ and $1-2q$.

On the basis of the model the change in the frequency of a in one generation can be calculated as

$$\begin{aligned} \Delta q = q' - q &= \frac{\Sigma(aa) + \frac{1}{2}\Sigma(Aa)}{\Sigma\Sigma} - q \\ &= \frac{-q^2(s-2c+sc+4cq-sq-csq)}{1+q^2(4c-s-cs)}. \end{aligned} \tag{1}$$

This conclusion can be made meaningful by considering its application to the special cases for which it would have the most interest.

TABLE 1

Approximate model of reproduction in a population with a rare deleterious recessive subject to reproductive compensation

CROSS	FREQUENCY	OFFSPRING			
		AA	Aa	aa	Σ
$AA \times AA$	$(1-2q)^2$	$(1-2q)^2$			$(1-2q)^2$
$AA \times Aa$	$4q(1-2q)$	$2q(1-2q)$	$2q(1-2q)$		$4q(1-2q)$
$Aa \times Aa$	$4q^2$	$q^2(1+c)$	$2q^2(1+c)$	$q^2(1+c)(1-s)$	$3q^2(1+c)+q^2(1+c)(1-s)$
Σ	1	$\Sigma(AA)$	$\Sigma(Aa)$	$\Sigma(aa)$	$\Sigma\Sigma$

Case 1. *No selection* $(s=0)$ *and no compensation* $(c=0)$.

Here equation (1) gives

$$\Delta q = 0$$

the expected solution for this situation.

Case 2. *Extreme selection* $(s=1)$ *and no compensation* $(c=0)$.

Here equation (1) gives

$$\Delta q = \frac{-q^2}{1+q}$$

the expected exact solution to the problem of selection against a recessive lethal (Li, 1955:252) without compensation.

Case 3. *Extreme selection* $(s=1)$ *and complete compensation*

$$c = \frac{s}{4-s}.$$

Here equation (1) gives

$$\Delta q = -\tfrac{2}{3}q^2.$$

Complete compensation is here defined as an increase in reproductive success that exactly compensates for the loss due to $\frac{1}{4}$ of the progeny being homozygous for a. The corresponding value of c is obtained by setting the total progeny of $Aa \times Aa$ equal to its value without selection, $4q^2$, and solving for c. With complete compensation, therefore, selection is about $\frac{2}{3}$ as effective as when compensation is absent.

Case 4. *Moderate selection* $(0<s<1)$ *and no compensation* $(c=0)$.

Here equation (1) gives

$$\Delta q = \frac{-sq^2(1-q)}{1-sq^2}$$

which is the expected solution for this situation (Li, 1955:254).

Case 5. *Moderate selection* $(0<s<1)$ *and compensation complete*

$$c = \frac{s}{4-s}.$$

Here equation (1) gives

$$\Delta q = \frac{-\frac{1}{2}sq^2}{1-\frac{1}{4}s}$$

an approximation indicating that selection, with small values of q and s, is about half as effective with complete compensation as in its absence.

The indications are, as expected, that selection is less effective in eliminating early-acting deleterious recessives when there is reproductive compensation than when compensation is absent. It should be emphasized, however, that even with complete compensation, the changes in gene frequency still have the same sign and order of magnitude. Compensation can not actually increase the frequency of a recessive gene adversely selected in the homozygote as it can in one type of selection against the heterozygote (Glass, 1950). This is true not only for small values of c and for complete compensation $(c = \frac{s}{4 - s})$, but even for what might be termed moderate *over-compensation*, an excess of replacements over those lost. There is evidence from Glass' work and elsewhere that over-compensation actually occurs. Even with rather extreme over-compensation, $(c = \frac{1}{2}$ for a lethal), changes in gene frequencies are still negative for all meaningful values of q.

The present study, therefore, fails to confirm Lewontin's conclusion that relatively high stable equilibria can result from selection and compensation alone. The disagreement stems from differences in the use of the compensation coefficient. Lewontin assumed it to be constant for all kinds of susceptible matings, and I assumed it to be a function of the mendelian ratios expected in different kinds of susceptible matings. My analysis would indicate that compensation can accomplish no more than an elevation of the equilibrium frequency of a gene subject to adverse selection balanced by mutation pressure. Even this effect is slight, however, compared to expectations that have been expressed. Bianco, *et al.* (1952), for example, suggested compensation as a possible cause for the high incidence of Thalassemia, a fatal childhood disease attributed to a (nearly) recessive gene, in certain human populations. Calculated gene frequencies are too high to be attributed to mutation pressures, even if the mutation rate were many times greater than the highest otherwise known for man. Taking $\frac{2}{3}q^2$ and q^2 as the rates of elimination of lethal genes with and without compensation, respectively, the mutation rates necessary to stabilize a given gene frequency would likewise be in the ratio of 2 to 3. Compensation is insufficient to account for gene frequencies that could not be maintained by mutation pressure.

It seems likely that the genetic effect of reproductive compensation might be difficult to detect, because there are so many other much more important factors operating. Inevitably, however, there must be some effect. One would expect the early developmental stages of man, and other species with highly developed parental care, to be somewhat less well purged of deleterious gene actions than comparable stages of species in which parental responsibilities end with the production of zygotes.

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ABILITY TO TASTE PHENYLTHIOCARBAMIDE AMONG ALASKAN ESKIMOS AND OTHER POPULATIONS

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IN 1932, Fox observed that some people could taste crystals of phenylthiocarbamide (PTC) placed on their tongues while others could not. Family studies carried out by Snyder (1932), Lee (1934), and Blakeslee and Salmon (1935) suggested that the character was genetically controlled, non-tasters being recessive for a gene, *t*. A number of human populations were tested with crystals or papers soaked in PTC, and differences in frequency of non-tasters recorded (see Gates, 1946).

Various methods were developed for determining the taste threshold for PTC, that is, the minimum concentration that can consistently be tasted by the subject (Blakeslee and Salmon, 1935; Hartmann, 1939; Falconer, 1947; Harris and Kalmus, 1949). The most satisfactory method is the sorting test of Harris and Kalmus, which has been quite widely used in anthropological work. This paper records the results of such tests on Alaskan Eskimos and summarizes what is thought to be reliable information on the distribution of the PTC-tasting in different human populations.

SUBJECTS AND METHODS OF STUDY

The subjects were 68 Eskimos from the Northern (Inupik) language group in Alaska. The majority (55) came from the large town of Barrow at the North of Alaska, and the remainder from the village of Beaver on the Yukon River. There was a preponderance of females (41), but no sex difference was apparent in the relatively small sample tested. Subjects varied in age from 8 to 71, the majority being young adults. Females are reported to have slightly lower PTC threshold than males, and young people a slightly lower threshold than old people (Harris and Kalmus, 1949; Barnicot, Harris and Kalmus, 1951). This difference should not materially affect the proportion of non-tasters recorded in the Eskimo population. In the test, serial dilutions of a 0.13% solution of PTC in boiled water are prepared. Harris and Kalmus originally used 13 two-fold solutions. The concentrations of PTC in the test solutions are given in table 1. Beginning with the lowest concentration, the subject is asked to taste a few cc. of the solution. When a concentration is reached which the subject can taste, he is presented with 8 cups, 4 containing the PTC solution and 4 boiled tap water. If these are correctly distinguished, he is tested with more dilute solutions until his threshold is determined. If he fails to identify the 8 cups, he is given a more concentrated solution until the sorting is correct.

TABLE 1

No. of solution	1	2	3	4	5	6	7
PTC concentration mg./l.	1300.00	325.00	81.25	20.31	5.08	1.27	0.32

RESULTS

The results of the present study are recorded in figure 1. Observations on some other populations made with the same technique are presented for comparison. The PTC taste thresholds of all populations show a clear bimodal distribution, with an antimode at solution 3. This was selected as the dividing point of tasters and non-tasters.

COMMENT

The proportion of non-tasters in the Eskimo population is relatively high (26%). The only other published investigation of Eskimos seems to be that of Sewall (1939) on the Eskimos of Labrador. This was carried out with test papers, and an even higher proportion of non-tasters (41%) was recorded. What appears to be reliable information on the frequency of the PTC-tasting character in different populations is summarized in table 2 and figure 2. The overall distribution of the character is beginning to appear, although large gaps remain to be filled in. The highest reported frequency of PTC non-tasters is in India (43%) with a somewhat lower frequency (27%) in Ceylonese Tamils.

Fairly high frequencies (25 to 32%) have been found in Western Europeans, while the Welsh have a somewhat lower frequency (17%). The frequency in the Eskimos is of the same order as in Western Europeans (26%), in sharp contrast to American Indians, who have very low frequencies. Very low frequencies are also found in East and West Africans. Intermediate frequencies are found in South-East-Asians, varying from 2% in Formosa through 7% and 11% in Japanese and Chinese to 16% in Malaya.

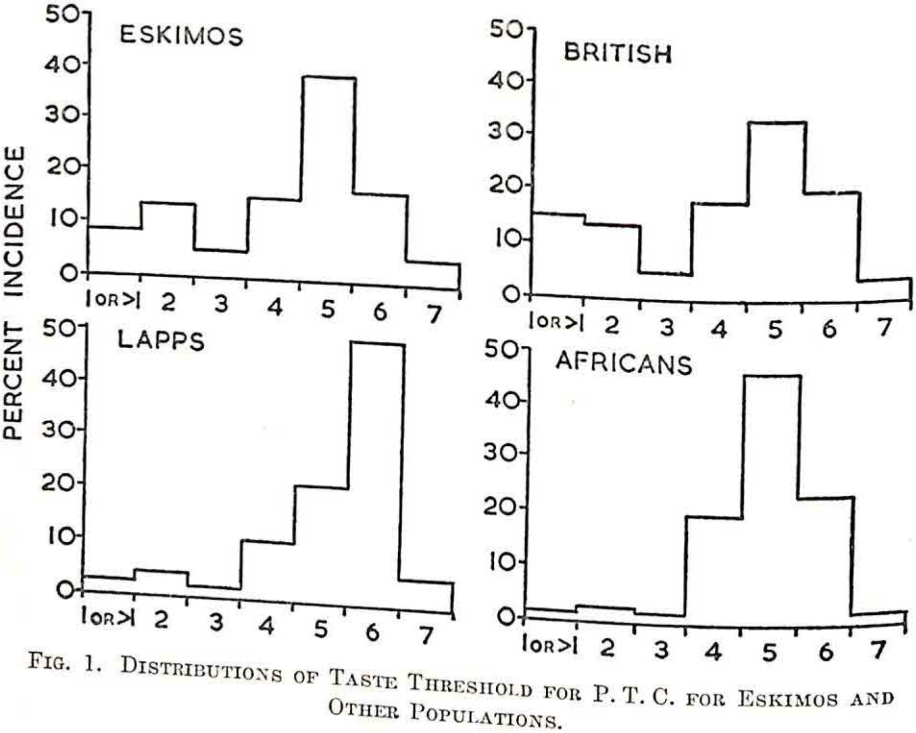


FIG. 1. DISTRIBUTIONS OF TASTE THRESHOLD FOR P. T. C. FOR ESKIMOS AND OTHER POPULATIONS.

It is worth following up clues which might lead to the discovery of selective agencies acting upon the PTC-tasting polymorphism. This is particularly true since the polymorphism appears to be a very long-standing one in evolutionary terms: Fisher, Ford and Huxley (1939) found in chimpanzees approximately the same proportion of non-tasters of PTC as is found in Western Europeans. This is not the case in some polymorphisms, such as β-amino-*iso*butyric acid excretion (Gartler, Firschein and Dobzhansky, 1956) and the serum haptoglobins (Blumberg, 1959), where all the human varieties of a trait do not appear in other primates.

TABLE 2

The incidence of non-taster of Phenylthiocarbamide and related compounds in different populations

POPULATION	NUMBER TESTED	PER CENT NON-TASTERS	TESTING METHOD	REFERENCE
English	440	31.5	Selection	Harris and Kalmus, 1949
U. S. White	3643	(29.8)	Crystals	Snyder, 1932
Danes	314	31.8	Selection	Mohr, 1951
Norwegians	266	30.5	"	Merton, 1958
Finns	202	29.2	"	Allison and Nevanlinna, 1952
Spaniards, Northeastern	306	24.8	"	Pons, 1955
Basques, Spain	98	(25.0)	Paper	Boyd and Boyd, 1937
Lapps, Norwegian and Swedish	140	6.9	Selection	Allison and Nevanlinna, 1952
Africans, mostly West African	74	2.7	"	Barnicot, 1950
Chinese (in London)	66	10.6	"	Barnicot, 1950
Chinese (in Malaya)	50	2.0	"	Lugg and Whyte, 1955
Japanese	295	7.1	Selection	Saldanha, 1958
Formosan " Natives "	1756	(1.8)	Crystals	Rikimaru, 1936
Malays	50	15.6	Selection	Lugg and Whyte, 1955
Tamils in Malaya	50	27.2	"	Lugg and Whyte, 1955
Bombay Indians	200	(42.5)	Solution	Sanghvi and Khanolkar, 1950
Ramah Navahoes	264	(2.0)	?Paper	Spickles (unpublished)
Cree & Beaver Indians N. Alberta	489	(2.0)	Crystals	Matson, 1940
Eskimos, Labrador	130	(41)	Paper	Sewall, 1939
Eskimos, N. Alaska	68	25.8	Selection	Present paper
Negritos, Malaya	50	18	"	Lugg, 1957
Aborigines (Sēnoi), Malaya	50	4	"	Lugg, 1957
Arabs, Assiut		(18)	Paper	Boyd and Boyd, 1937
Bantu, Kenya	208	3.8	Selection	Allison, 1951
Arabs, Kenya	63	25.4	"	Allison, 1951
Indians, Brazil	163	1.2	"	Kalmus, 1957
Welsh	203	17.24	"	Beach, 1953
Portuguese	454	24.0	"	Cunha and Abren, 1956

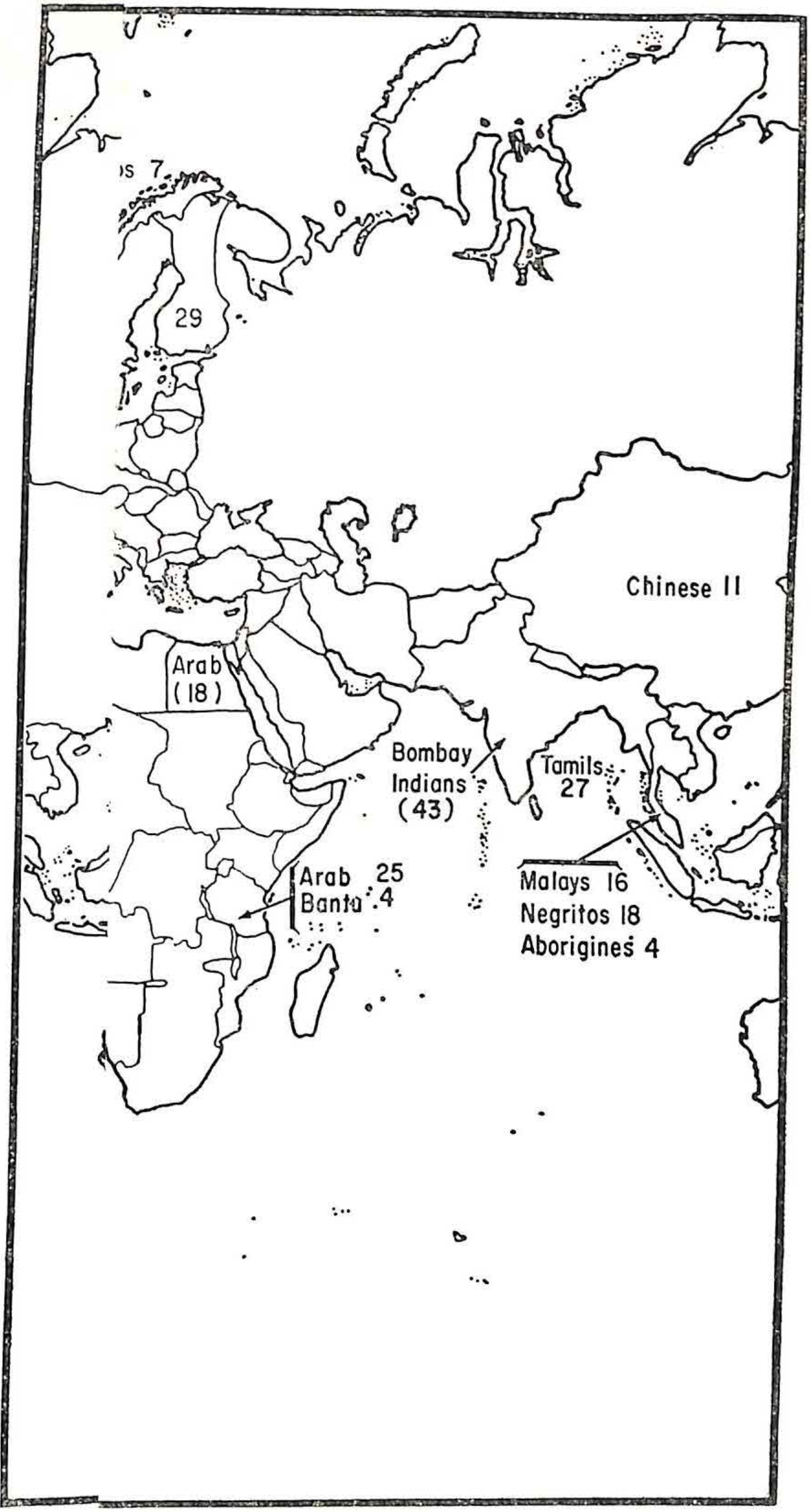
Parentheses indicate studies in which techniques other than selection were used.

Various compounds have been shown to be tasted by PTC-tasters and not by non-tasters, although the threshold concentration is variable (Hopkins, 1942; Harris and Kalmus, 1949; Kuhn and Zilliken, 1950; Barnicot, Harris and Kalmus, 1951). A characteristic of these chemicals was the presence of the thiourea grouping $=\mathrm{N}-\underset{\underset{\mathrm{S}}{\|}}{\mathrm{C}}-$. Substitution of oxygen for sulphur removes the difference in response of PTC-tasters and non-tasters. The grouping which is specifically concerned with taste-threshold bimodality is common to a large series of anti-thyroid and goitrogenic substances (Riddle and Wybar, 1944; Astwood, Bissell and Hughes, 1945; Boyd, 1950).

Following up a possible relationship to thyroid diseases, Harris, Kalmus and Trotter (1949) reported a significantly higher incidence of non-tasters among persons with nodular goitre than normal controls; the association was not found in persons with diffuse goitre. These findings have recently been confirmed by Evans and Kitchin (1958).

In examining the overall distribution of the PTC-tasting character, then, it is worth considering whether it might be subject to environmental influence and, in particular, whether it bears any relationship to thyroid function. The first question which arises is whether any obvious correlation with extremes of climate exists. There seems, in fact, to be no such correlation. The highest and lowest frequencies of non-tasters are recorded in tropical climates (Indians, Africans, and South American Indians, respectively). At the other extreme, Eskimos have rather high frequencies of non-tasters while Lapps have rather low frequencies. The second question is whether there is any correlation with intake of iodine or goitrogenic compounds. Available data are insufficient to reach any decision on this point. It would seem to be worthwhile to investigate the PTC-tasting frequencies in populations living for long periods in iodine-deficient regions or those in which goitrogenic compounds are regularly consumed. Clements and Weshart (1956) have discussed the distribution of goitrogenic substances in some diets.

The considerable difference in PTC-tasting frequency between Eskimos and American Indians appears also in other genetically controlled characters, including urinary β-amino-*iso*butyric acid excretion (Allison, Blumberg and Gartler, 1959), haptoglobin types (Blumberg, Allison and Garry, 1959), and some blood groups (particulary the near absence of type B in Indians and its presence in Eskimos) (Corcoran, Allen, Allison and Blumberg, 1959). These observations together with linguistic


s 7
29
Chinese 11
Arab
(18)
Bombay
Indians
(43)
Tamils
27
Arab 25
Bantu 4
Malays 16
Negritos 18
Aborigines 4

and cultural differences between American Indians and Eskimos, make it improbable that the two populations are closely related racially.

SUMMARY

Using a method for determining taste thresholds for PTC, 26% of 68 Northern Alaskan Eskimos were found to be non-tasters. This is in sharp contrast to the low frequencies of non-tasters reported in American Indians.

The overall distribution of non-tasters of PTC is discussed. In view of available evidence suggesting a relationship of PTC to goitrogenic compounds and thyroid disease, it is possible that these frequencies might be subject to environmental influence. No obvious correlation with extremes of climate appears to exist, but it would seem worthwhile to investigate the possibility of a relationship to intake of iodine and goitrogenic compounds in various populations.

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We are indebted to various people who facilitated the field work in Alaska, in particular Mr. M. C. Brewer and Dr. J. Halvorsen, Barrow. The authors are indebted to Dr. J. Tata for his comments on the manuscript. The participation of one of us (A. C. A.) in the Alaskan investigation was made possible by a grant from the Wenner-Gren Foundation for Anthropological Research.

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BOOK REVIEWS

Handbook of Population Census Methods. Volume I. *General Aspects of a Population Census.* vi + 164 pp. Volume II. *Economic Characteristics of the Population.* vi + 79 pp. Statistical Office of the United Nations, Studies in Methods, Series F, No. 5, Rev. 1. United Nations, New York, 1958. Price not available.

The major problems of research in comparative demography are not those of method, personnel, institutional facilities, or financial support. They are, rather, the paucity of data and the selectivities in the data that do exist. Little can be done to correct the situation with reference to the past record, but the Statistical Office of the United Nations is the leader among many international groups stuggling heroically to persuade nations to collect and process more and better statistics in the future. The fundamental activity in the statistical field is the census, and here major attention is focused, particularly in the years around the end of each decade.

The Statistical Office issued a series of manuals in connection with the national censuses taken in or around 1950. These were based on the experiences of countries with censuses from 1935 to 1944. A new three-volume *Handbook of Population Census Methods* is being issued to include the international recommendations for the 1960 censuses and to utilize the experiences of the censuses taken in the years from 1945 to 1954. In addition, the discussions are recast with reference to "the problems and needs of countries having little experience in and few resources for census-taking" (Volume 1, p. iii.). The needs of the country are given priority over international comparability. Definitions of a census and an enumeration are tolerant, and many sections are presented so that they can be models for procedures. Sampling is emphasized, including the possibility of a sample survey if a complete census is not possible. Census schedules are reproduced for Canada, Ceylon, France, India, Japan, Pakistan, Panama, Senegal Territory, Tanganyika Territory, Uganda Territory, United States, and Yugoslavia.

The *Handbook* is obviously essential for those who are involved in census activities. It is a valuable document for research consumers of census data from countries with little census experience, for here is the international model and the guide to definition and procedure. For students of international cooperation in scientific advance, it is a fascinating document.

The publication of the *Handbook of Population Census Methods* and the various preceding and associated documents is an indication of major progress in the extension of census activities, but it is an unsafe guide to the measure of that progress. The statement that 150 countries took censuses and secured data on more than two billion people in the decade from 1945 to 1954 suggests an approaching great leap forward for regional and comparative demographers. Unfortunately the two billion people include the 600 million people of China (as reported from the "census") for whom only a few figures on the sex ratio, the populations of provinces and cities, and the numbers of the minority groups have been published from a quick survey that was in large part a registration of heads of households in association with a registration of voters. If the figures on countries and populations included only those covered by reasonably complete and accurate censuses, the data from which were published, the global status would warrant little optimism. The state's role seems to require the taking of a census, or at least the statement of intent to take one. The motivations and the resources for tabulation, analysis, and publication are more restricted. The realization of this hiatus between enumeration and publication is widespread; so also is the appreciation of the qualitative deficiencies of the data that are collected and the inadequacies of the tabulations that are made. Perhaps the most significant developments with reference to the 1960 censuses are the activities of countries and international organizations oriented toward the analysis of the data that will be collected.

If further mitigation of research optimism is required, it follows from an inspection of Table 1, Volume 1, on population censuses taken in selected countries during the period from 1855 to 1957. The list would be reduced very greatly if censuses taken but completely or largely unpublished were deleted. The definition of a census in precise terms would reduce the list of countries and the frequency of censuses for the countries that remained, even if no requirement of publication were imposed. For instance, the statistical activities of the Manchus in the last few years of their reign are listed as three censuses of China; the activities of the Republic of China in 1912 and 1928 yield two censuses; the activities of the government of Communist China in 1953-1954 yields another census. Thus there were 6 censuses of China between 1905 and

1957! The listing of the initial *koseki* registration of 1872 and the registration compilations of the years from 1898 to 1918 give Japan 6 "censuses" in its precensus period. However, there are instances in which the facts are more optimistic than the list. The 1895, 1915, and 1940 censuses of Taiwan are omitted. Moreover, countries such as Korea with substantial census records are omitted entirely.

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The Ecology of Human Disease. By JACQUES M. MAY. xxiv + 327 pp. MD Publications, New York, 1958. $7.50.

"The concept of medical ecology formulated by Dr. May conceives disease as a convergence in time and space within the person of the patient of environmental stimuli (organic, inorganic, or sociocultural). These stimuli are a challenge which induces a tissular response, that is, disease (communicable, degenerative, or behavioral), which in its turn eventually results in ecologic adaptation and survival or in total maladjustment and death." In these words Martí-Ibáñez summarizes in his Foreword the position taken by the author, which is sufficiently in accord with Haeckel's original definition of ecology to be acceptable, and sufficiently stimulating to be a useful extension of its customary scope.

The first seven chapters explain the author's position in general and simple terms; but so simple and general that eyebrows are sure to rise at some of the omissions or over-simplifications. Of the three groups for whom the author is writing—physicians, geographers, and anthropologists—two will find little that they did not already know, and some statements that they will question. The position that *all* disease results from the interaction of external stimuli and tissue response is probably defensible from a philosophical point of view, but one may hesitate to relinquish in practice the category of disease in which disorder appears to arise from innate derangements involving, but not dependent upon, environmental conditions. It is possible that this category will progressively shrink as knowledge increases, but it seems premature to discard it at this time.

Specialists may be somewhat irritated at the casual manner in which the author brushes aside existing knowledge in some fields; *e.g.*: "Little is known about the direct action (of climate on man)"; and "no comparative knowledge of physiology in human groups is available to this day." While an author has a right to define his terms as suits his argument, readers will not be very happy with the statement, "Culture is the sum total of the concepts and techniques that human groups use and abide by in the environment in which they are placed, in order to survive"; and there may be many protests against the statement that anthropology *includes* anatomy, craniology, archeology, and several other highly respected disciplines, with more than a hint that medicine has now been admitted. It is difficult to cite characteristics that will unequivocally assign an individual to a "race," but confusion is hardly less confounded by substituting an areal population as the classification unit. The presence of *Clonorchis*, for example, carries very strong presumptive evidence that the host has at some time been in the Far East, but whether as casual visitor or long-time resident, and therefore whether a true member of the geographic group or not, is in no way revealed.

In the remaining thirteen chapters the author has brought together a wealth of information on the incidence of disease, its geographic history, and associated environmental factors (physical, biological, sociological). He has presented it in very readable form. Undoubtedly this presentation may be called ecological, and it represents a very good first approach to an important and sizeable task; but readers will hope that ecological descriptions of the future will get down to more intimate details of the controlling factors, their relative intensities, and some quantitative expression of their interaction.

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Blood Groups. A. E. MOURANT, Chairman of the Symposium. Brit. Med. Bull., *15*(2):89-174, May, 1959. $3.25.

Some of the leading British investigators assembled recently to discuss such subjects as blood group substances and antibodies, heredity of the blood groups, erythroblastosis fetalis, seed agglutinins, animal blood groups, blood groups and disease, etc. The papers have now been collected in a special issue of the British Medical Bulletin. The bulletin constitutes in fact a monograph on recent advances in blood grouping and as such is indispensable to all concerned with this subject.

In general, the work presented is on a high plane and can be accepted without reservation. However, in this publication as in other recent books on the subject published in England, there is a tendency to emphasize the contribution of British investigators. In some cases this has been carried to an extreme, with two unfortunate results. Firstly, many of the original observations had been made by scientists in the United States and later were confirmed by workers in England, but the original work is not cited, even though in many instances it was more complete. As examples may be mentioned Wiener's pioneering work on the heredity of the Rh-Hr blood types, and his early studies on variants of the Rh-Hr factors especially the **Rh₀** variants. There has been a tendency to credit such work incorrectly to British workers. For example, a recent textbook in hematology incorrectly credits Fisher, a biometrician and *not* a serologist, with the discovery of the various Rh factors and their heredity, while Stratton is incorrectly credited with the discovery of **Rh₀** variants, even though his only contribution was to coin the alternative fallacious symbol D^u. Secondly, no reference is made to the work of other leading investigators whose findings and conclusions are at variance with certain views held by British investigators regarding basic questions in the field. This one-sided presentation by British authors limits the usefulness of the symposium. The chief serious fault of the presentations is that they give only a single point of view, and fail to discuss or even admit the existence of any evidence for any alternative viewpoint.

The unfortunate effects of the failure to consider the contributions of other investigators is illustrated by the vast amount of effort and time that British workers have wasted on blood groups and disease, a topic to which considerable space is devoted in the monograph. Of course, the clinical importance of blood groups in erythroblastosis fetalis is now universally recognized, following the pioneering work of Levine and his associates in the United States. Here a definite mechanism has been discovered, namely, isosensitization by pregnancy (or transfusion), with subsequent transplacental transfer of maternal isoantibodies to the fetus, resulting in hemolytic disease. This correct basic concept has not only served to explain the pathogenesis of erythroblastosis fetalis, but also has led to a rational treatment. (Here again, the otherwise excellent articles on erythroblastosis fetalis in the Bulletin fail to mention the pioneering work of American investigators.)

On the other hand, no one has ever been able to suggest any mechanism by which, or any reason why a *group O* person should be more susceptible to duodenal ulcer, or a *group A* person to gastric carcinoma or fractured femur, etc., any more than why it should matter whether the iris of one's eyes is colored brown, gray, or blue, so long as it is pigmented. The answer, of course, is that "the hardest things to explain are those which aren't true." No mention is made of the fact that this line of investigation was popular more than three decades ago, when the subject was young and knowledge and understanding limited, but was abandoned when it was realized that such studies lack any sound rationale and have no direction. It seems that the modern more sophisticated approach to the fallacious problem of blood groups and disease originated from a pronouncement made by a biometrician that "no gene is neutral in natural selection." This proclamation was taken to heart by many investigators, although no one has ever disproved the existence of neutral genes. The effect of this edict has been to stimulate an endless series of studies on A-B-O blood group distribution in such diseases as peptic ulcer, carcinoma of the lungs and stomach, pituitary adenoma, pernicious anemia, fracture of the femur, dental caries, etc. And this represents barely a beginning, since nothing or hardly anything has yet been done regarding the M-N-S types, Rh-Hr types, P types, eye color, hair color, etc., which are gene determined, and, therefore, according to the biometricians, are not neutral in selection.

One of the many fallacies of the studies on blood groups and disease is that the various series of patients are collected from case histories over a period of many years from a number of different hospitals, while the so-called controls are generally a series of consecutive blood donors assembled in a brief period, so that the two series are not comparable. Moreover, when the A-B-O blood groups distribution is studied in many different diseases, some of the series are bound by chance to show deviations at the conventional level of so-called statistical significance. Finally, there is always the possibility of the introduction of some sort of bias into such investigations. For example, Billington failed to confirm the supposed higher incidence of carcinoma of the stomach among *group A* persons but reported instead that the distribution of the groups appeared to differ according to whether the lesion was in the prepyloric area, the body of the stomach, or the cardiac area. This led Wiener to inquire (Letter to the Editor, Lancet, Dec. 22, 1956) whether "in assigning the cancer to one or the other area, the investigator was subconsciously biased, in borderline cases, by his prior knowledge of the patient's blood group." Billington responded, "I admit I am unable to exclude the possibility that the figures in relation to the site of gastric carcinoma might have been influenced in borderline cases by knowledge of the patient's blood group." One wonders, why in the review on blood groups and disease in the Bulletin no reference is made to this very important fact. Similarly, the Bulletin cites the first study on blood groups in pituitary adenoma but fails to mention the later more critical paper refuting the original report. With regard to the excess of *group O patients* among individuals with duodenal ulcer, one must bear in mind that the cases were culled from hospital records. One of the most common causes for hospital admissions in such cases is hemorrhage, and in emergencies such patients are given group O blood when there is not enough time for grouping and matching tests. This could have caused confusion regarding the patient's true blood group, and such cases might have been classified erroneously as group O, thus accounting for the bias.

In summary, while the issue of the British Medical Bulletin devoted to the Blood Groups is indispensable for workers in the field, it cannot be recommended for non-specialists because of the parochial nature of its presentation and some misinformation it contains.

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Manual de Antropología Física. By JUAN COMAS. 698 pp., 114 figs., 98 tables. Fondo de Cultura Económica, México 12, D. F., 1957. No price indicated.

For a long time there has been a great need for a textbook of physical anthropology in Spanish or Portuguese, the languages spoken in Latin America. Comas' book attempts to supply this need.

It starts with a chapter on Generalities (62 pp.); then come: Origin and Evolution of Man (38 pp.); Heredity (74 pp.); Growth (64 pp.); Somatology (57 pp.); Biotypology (42 pp.); Craniology and Osteology (71 pp.); Paleoanthropology (104 pp.); Racial Systematics (57 pp.); and Utilization and Teaching of Physical Anthropology (41 pp.). There are also seven appendices (23 pp.), a good bibliography and several indexes.

The book is well written and the author is to be congratulated for the emphasis on the "modern approach" in physical anthropology. Another good feature is the abundance of references in the footnotes and in the bibliography at the end of the volume. The printing is excellent, and there are few typographical errors.

In a work of such a wide coverage, some mistakes and omissions are inevitable. Some of them will be discussed here. But it should be clear at the outset that the book, even in the present form, is to be highly recommended to all persons interested in these problems.

Our observations can be summarized as follows:

a) The chapter on Generalities is very good, especially the detailed historical section.

b) Chapter 2 presents an informative review of the concepts about the origin of life, organic evolution and, at the end, of human evolution. As this latter subject is discussed at length in chapter 8 (pages 497-525), it seems that it would be better to discuss only organic evolution in chapter 2, leaving all the material concerned with human evolution for discussion in chapter 8.

c) Being a geneticist, the reviewer was especially interested in the chapter on Heredity. In the beginning of the chapter the difficulties encountered in the study of human genetics are stressed, but not a single word is given about the advantages of the human material. Mendel's first law is presented in reference to rats (why?). A little more emphasis should be placed on the notion of the separation of genes, as opposed to the old idea of blood mixture.

Pages 127-130 include special, though not really important, cases of modifications of Mendelian ratios. This part could be omitted in a book of this nature. Sex-linkage inheritance is exemplified first in *Drosophila*, and then in man (hemophilia).

On page 134 there is a list of human normal characteristics (taken from Guyénot, 1942, *L'hérédité*), stated to be inherited in a simple way. However, in the great majority of the cases their hereditary mechanisms have not yet been completely worked out and their inheritance is probably due to several genes (for example: eye color, hair shape, stature; as to the cephalic index, it was shown that identical indices might result from quite different conformations of various cranial bones, which in turn might be due to entirely different genes).

On pages 143-146 he assumes that all geneticists are advocates of Mutationism. Actually, the great majority of them are neo-darwinists.

On page 147 he says that Simpson does not consider mutations as an important factor in evolution. Being a neo-darwinist, Simpson does consider mutations important, since they will contribute to the variation upon which selection will act.

The great omission in this chapter concerns the fundamental principle upon which all population genetics is based: the Hardy-Weinberg law.

d) The chapter on Growth contains a critical appraisal of the indices and measures which are being used in these studies. The value of the comparative material presented in the tables of this chapter would be greatly increased if the author had included the number of individuals studied in each series. As the tables stand now, it is difficult to evaluate the significance of the differences presented.

e) It would be better to include the section about cranial sutures (in chapter 7) after the description of the craniometric points.

f) In chapter 8, the Hylobatidae are considered as a family on page 440, but on page 436 they are presented as a sub-family of the Pongidae. On page 491 the author says "as we have already seen when dealing with racial systematics"—but the chapter on Raciology is *after*, not before chapter 8. And, it is believed, the section about the proofs of human evolution (page 522) should be placed before this subject is discussed (page 497).

g) The chapter on Racial Systematics would be greatly improved by the addition of figures presenting the distinguishing features of individuals from the several groups discussed, as well as some maps of their distribution. The section on Demography is so small (pages 582-583) that it is hardly of any value.

h) In the first part of chapter 10 (Utilization and teaching of Physical Anthropology), the author seems to consider as synonyms the terms "physical anthropology" and "anthropometry," a view which is contradicted by the very nature of his book!

All the criticisms involved minor points or rearrangement of the materials presented. Comas has written a readable, up-to-date review of the fundamental concepts of physical anthropology. It is expected that his book will enhance significantly the progress of this science in Latin America.

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Recherches sur L'Anthropologie des Françaises. By SUZANNE DE FELICE. 316 pp. Masson, Paris, 1958. No price indicated.

In this thorough and careful monograph, Dr. de Felice has attempted to provide us with the same sort of somatological information concerning French women as the so-called older school of physical anthropologists have given us concerning the adult male sector of the population of many tribes and nations. It has the virtues and deficiencies inherent in this approach to anthropological knowledge, as well as some virtues of its own. We have far too little precise data on women, and any addition is valuable and interesting. Fortunately, a few other such studies exist, and one of the most useful aspects of Dr. de Felice's work is her comparison of the French with other European female groups which have been examined. The similarity, in size and proportions, between the means of all these groups, ranging from Slovenes and Esthonians to Norwegians and Swiss, is remarkable. Differences, in the frequency of blondness of eyes, hair and skin are, of course, demonstrated, and from other sources we know that differences in such genetic characteristics as blood-type frequencies are many times greater.

But the volume lacks data on matters such as this, as on other genetic characteristics. Nor is there any information concerning physiology, or growth, or aging. The group studied consists of 140 women in Paris, who were willing to submit to the author's examination, in good health and between the ages of 20 and 48 years. The ancestors of the subjects are said to have come from all different parts of France, as one might expect in a capital city, but the proportions derived from the various sections of the country are not stated in any quantitative manner. Only one-quarter of the women were mothers and the greater number were nurses or student nurses. One may doubt whether such a sample is either random or properly stratified to represent the women of France. Obtaining an adequate sample of women which is demonstrably representative of an ethnic group for anthropological examination in the nude is rendered difficult if not simply impossible by the inhibitions of our culture.

The author is to be commended for the complete frankness with which she discusses her difficulties and uncertainties, for the care with which she has described her techniques, and for the precision and clarity with which she has presented not only her own data, but the comparative material which she has included. Her discussion of female characteristics is particularly valuable and she has devised an index (p. 69) to express the degree of separation of the breasts. In the opinion of this reviewer, however, a greater number of visual observations could well have been included. It would doubtless be out of the question to record, correctly, the degree of curliness of the hair, even of females who were willing to submit to an examination of this sort, but hair texture might have been mentioned. The shape of the nose should, in a study of this nature, have been more adequately described. The use of skin-fold calipers would have assisted in the description of adiposity. Eyebrows are not even mentioned.

Among items of interest in this study are the lack of any significant correlation between stature and cephalic index, the lack of greying in pubic hair, the lack of consistent trends in the age-differences recorded, (*n. b.*, on pages 202-205 the younger and older age-groups are labelled in reverse, by misprints at the top of each column), and the almost total absence of red hair among the Frenchwomen included in the series studied. For the body as a whole, weight and the position of the breasts appear to show the greatest degree of variability; for the face, the shape of the lips and of the nose are most variable. On the whole, however, standard deviations are lower than those of the other European groups with which this French series is compared. One may still doubt, however, that the French are in fact more homogeneous biologically than other European nationalities. Despite its lack of theoretical conclusions, this monograph should serve as a most useful source of information concerning the physique of European females of the present generation.

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Primates: Paléontologie Humaine. By JEAN PIVETEAU. *Traité de Paléontologie,* Tome 7. Pp. 675. Illustrations 639. Paris, Masson & Cie., 1957. Francs 12,800.

This is the seventh and final volume of the magnificent Traité de Paléontologie edited by Professor Jean Piveteau, the present volume being entirely the work of Piveteau. It triumphantly and fittingly caps the preceding volumes. Piveteau's *Primates* is by far the best and most exhaustive work on the paleontology of the primates available in any language, and it will serve as an indispensable work of reference for many years. The text is beautifully clearly written, and the 639 illustrations are a veritable feast, including an extra dividend of 8 plates, 4 of them in color. The bibliographies following each chapter are excellent, and there is a good index.

Although this is a work on human paleontology, it covers the paleontology of all non-human primates, the first third of the book being devoted to them. The remainder of the book deals with the process of hominization as revealed by the paleontological record. The reader is not far into the book before he discovers that what he is reading is much more than a human paleontology. It is that, but also a great deal more, for this is a work on the origin and evolution of man in which not only is virtually every find described (there are some omissions) and classified, but its relation to other forms is clearly set out, and all to the end of throwing light upon the problem of the manner in which man came to be as we find him today. It is all remarkably well done and extremely readable to boot. It is a pity that the price of the book will make individual ownership of it a rarity. Most readers will have to borrow it from a library. Somehow in the English-speaking world we manage to produce books much less expensively than the French and the Germans. I wonder why?

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Behavior and Evolution. Edited by ANNE ROE and GEORGE GAYLORD SIMPSON. 557 pp. Yale University Press. New Haven, 1958. $10.00.

Evolution is a general theory of biology. It represents a synthesis of the facts of behavior of organisms as well as those of structure (morphology) and organ function (physiology), for these three aspects are inseparable. The desirability of an interdisciplinary appraisal of their bearing on evolution led to two symposia and, eventually, to the present book.

The relationship between evolution and behavior turns out to be a complex one. Since our knowledge in both fields is still very imperfect, the different participants are, understandably, unable to present a definitive position. Bullock faces the issue squarely when he says that physiological mechanisms cannot account for observed behavior: "Hope lies in the discovery of new parameters of neuronal systems." The aims of the volume are more modest: the localization of facts, theories and techniques in each of the pertinent disciplines that may be applicable in other fields. The bulk of the book is perhaps best considered as a documentation of the intricate relationship between evolution and behavior.

The complexity of the subject is indicated when behavior is approached from the facts of biology. The comparative ethologists point out that one can use the data from comparative behavior in the same way as one uses the findings of comparative anatomy and physiology. Mayr for instance mentions the reclassification of finchlike birds on the basis of their behavior. The same conclusion was reached independently on the basis of morphological criteria. On the other hand behavior does not always correlate with structure as expected. For instance the cat has the retinal and cerebral structures (in its lateral geniculate body) that are always associated with color vision, but the cat is totally color blind. The same complexity exists for the physiological mechanisms. Beach, after reviewing the endocrinological correlates of sexual behavior, concludes that the specific nature of the hormone is only part of the mechanism underlying sexual behavior in the male animal. Evolution in this kind of behavior is more likely attributed to changes in "physiological systems upon which hormones tend to exert their effects. A prime candidate for intensive consideration and study is the nervous system."

The student of behavior, with his "functional" approach, is likely to be interested in ontogenesis and, in general, in adaptation of organisms to immediate conditions rather than in the long-range adaptations associated with phylogenesis. The functional concepts when applied to the evolution of behavior may become awkward or vacuous. Emerson, for instance, speaks of evolution occurring in the direction of improved homeostasis. This position, a little later on, forces him to indicate that "Homeostasis often is the maintenance of unbalance, disequilibrium and asymmetry, as well as the reverse" (p. 323). Such usage of physiological concepts does not add to a meaningful description and analysis of behavior.

The concepts used in relating behavior to evolution are in need of clarification. Pittendrigh in a very fine paper performs this function for the concepts of adaptation and natural selection. He points out "that adaptation is a relative matter in many senses; that it cannot be discussed in absolute terms; and specifically that adaptation cannot be recognized with a criterion of necessity-for-viability."

The chapters dealing with behavior at the social and cultural level are perforce the least satisfying parts of the book. As Huxley remarks, "Through human culture, behavior has reached a supraorganismal level" (p. 454). The biological theory of evolution does not seem to be very relevant to behavior at this level. Yet the book, when considered in its entirety, does present a valuable range of documentation of the relationship between evolution and behavior.

The volume includes twenty-three chapters collected into five major areas. The editors have supplied appropriate and brief introductions to each part of the book. Two of the chapters were written by Simpson especially for this volume. The first article is a rapid and clear review of theories of evolution. In the epilogue the author attempts to gather together into a meaningful whole the diverse strands of thought and of fact found throughout the book.

The articles vary greatly in the demands that they make on the reader. Articles such as Simpson's discussion of theories of evolution or Bullock's treatement of *Evolution of Neurophysiological Mechanisms* can be profitably read by biologists or psychologists not trained in these areas. The same applies to most of the chapters on behavior. Other articles, however, can be fully appreciated only by specialists in the pertinent areas. For instance Pribam's article on the *Comparative Neurology and the Evolution of Behavior* is difficult going for anyone but the specialist in neurology.

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Mind and Matter. By E. SCHRÖDINGER. 104 pp. Cambridge University Press, 1958. $2.75.

This small book written by an outstanding physicist deals in a few pages with vast biological and psycho-physiological problems. In the first chapter (15 pages) the author considers consciousness, ethics, evolution and nationalism. Problems of evolution, particularly in how far the labors and strivings of the individual can be utilized for the development of the species, are discussed in the second chapter, which ends in some proposals for social reform in the machine age. The fourth chapter (17 pages) is concerned with the mind. For the physiological aspects of this problem Schrödinger quotes Sherrington extensively, but it seems strange to this reviewer to base one's discussion of this matter on Sherrington's experiments published in 1906 in spite of the enormous growth of neurophysiological knowledge in the last decades. Under the title "The Mystery of the Sensual Qualities" some remarks on the role of perception in science and the relation between physiological events and sensation and perception are made. This reviewer failed to see any notable advance over the ideas expressed by du Bois-Raymond in a famous oration in 1872. The book is apparently translated from German. Some sentences make no sense ("Now in man something is, so it seems, on the way that is not infrequent").

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BOOK NOTICES

COMMEMORATION OF THE CENTENNIAL OF THE PUBLICATION OF *The Origin of Species* BY CHARLES DARWIN. Proc. Amer. Philos. Soc., Vol. 103, No. 2. The American Philosophical Society, Philadelphia, 1959, $1.00. A group of outstanding investigators assembled at the Annual Meeting of the Society (April 1959) to reexamine Darwin's work and influence, with special reference to the theory of evolution and closely related problems (variation, genetic and non-genetic; hybridization; isolation; the nature of the fossil record).

THE MOLECULAR BASIS OF EVOLUTION. By Christian B. Anfinsen. xiii + 228 pp. Wiley, New York, 1959. $7.00. "The writing of this book has been stimulated by the excitement and promise of contemporary protein chemistry and genetics and by the possibilities of integration of these fields toward a greater understanding of the fundamental forces underlying the evolutionary process."

STATISTICAL METHODS IN BIOLOGY. By Norman T. J. Bailey. ix + 199 pp. Wiley, New York, 1959. $4.50. Designed to provide workers in the biological and medical sciences with an elementary account of the chief statistical methods. Contains useful "Notes on computing and calculating machines," and, as an appendix, a Summary of Statistical Formulae to be used as a quick reference guide.

EVOLUTION OF NERVOUS CONTROL FROM PRIMITIVE ORGANISMS TO MAN. Edited by Allan D. Bass. vii + 240 pp. The American Association for the Advancement of Science, Wash., D. C., 1959. $5.00. The problem is approached from the viewpoint of a botanist, an embryologist, a neurologist, a physiologist, a psychologist and a psychiatrist. In its totality, "this book portrays the impact of the evolutionary process upon the brain" (Bernard B. Brodie's Preface).

PRINCIPLES OF GENERAL PHYSIOLOGY: VOL. I. The Physico-Chemical Background. By L. E. Bayliss. xiii + 520 pp. Longmans, Green and Co., N. Y., 1959. $11.50. The "Principles," a classic in its field, has been given loving care by the Bayliss family for half a century. The first edition by Sir William Bayliss came out in 1915. The present fifth edition, retaining the traditional excellence, is the work of Sir William's son.

EXPERIMENTATION IN MAN. By Henry K. Beecher. 80 pp. Thomas, Springfield, Illinois, 1959. $3.50. Concerned with the ethical and legal aspects of human experimentation. Sets down views, concepts, and "rules," so that the investigator troubled by a given problem can find references to past thinking on the subject.

PHYSIOLOGY AND BIOCHEMISTRY. By George H. Bell, J. Norman Davidson, and

Harold Scarborough. xi + 1065 pp. Williams and Wilkins, Baltimore, 1959. $12.50. Fourth edition in a span of less than one decade. Published also in Italian and Spanish (1959). The fact that a physiologist, a biochemist and a clinician collaborated in producing this volume may have something to do with the balanced presentation of the material (and the success on the book market).

HISTORY OF BIOLOGY. By F. S. Bodenheimer. 465 pp. Dawson, London, 1958. $5.88. A boon to the student and teacher of the subject but not a textbook. Instead of a General Introduction, Part I consists of " Parerga and Paralipomena " (accessories and matters usually left out). Part II is a short factual history of biology. Part III consists of Sources from the History of Science—brief selections from 130 authors.

ISAGOGAE BREVES (A SHORT INTRODUCTION TO ANATOMY). By Jacopo Brengario da Carpi (ca. 1460-1530). Translated, introduced and annotated by L. R. Lind with anatomical notes by Paul G. Roofe. xi + 228 pp. The University of Chicago Press, Chicago, 1959. $5.00. Provides, in the words of Horace W. Magoun, a valuable transition between Charles Singer's translation of Mundinus' *Anatomy*, representing the medieval period, and Lind's earlier translation of the *Epitome* of Vesalius, representing the high renaissance.

PSYCHOPHARMACOLOGY: PROBLEMS IN EVALUATION. Edited by J. O. Cole and R. W. Gerard. viii + 662 pp. The National Academy of Sciences, Wash., D. C., 1959. $6.50. The subtitle should continue: Evaluation of Pharmacotherapy in Mental Illness. Focused on the assessment of the present (1956) state of knowledge by scientists engaged in research related to psychopharmacology.

REPORT OF THE MEDICAL RESEARCH COUNCIL FOR THE YEAR 1956-1957. By the Committee of Privy Council for Medical Research. Her Majesty's Stationery Office, London (available from British Information Services, New York), 1958. $1.82. " Genetics of Eye Disease " (pp. 50-56) is discussed in the section concerned with general trends in medical research. There is an informative summary of the work of numerous research institutes—the best available source of information on current biomedical research in Great Britain—and a classified list of publications.

ELEMENTARY STATISTICS WITH APPLICATIONS IN MEDICINE AND THE BIOLOGICAL SCIENCES. By Frederick E. Croxton. 376 pp. Dover Publications, New York, 1959. $1.95. A reprint (first edition, 1953) of a handy manual.

THE LIFE AND LETTERS OF CHARLES DARWIN. Edited by his son, Francis Darwin. Volume 1—xxii + 588 pp. Volume 2—562 pp. Basic Books, New York, 1959. Set $10.00. A reissue, with a foreword by George Gaylord Simpson.

ANTROPOMETRIA DA TRIBO CACONGA. By J. H. Santos David. 114 pp. Companhia de Diamantes de Angola, Publicações Culturais No. 36, Lisboa, 1958.

No price indicated. The author is a physician associated with the Diamond Company of Angola (Portuguese West Africa). He wrote about blood groups (1949) and studied the anthropometric characteristics of the natives of the region of Lunde and Songo (1955). This is a continuation of his anthropometric (largely osteometric) researches, devoted to the Caconga tribe.

TEN STEPS INTO SPACE. x + 202 pp. Monograph No. 6. Franklin Institute, Philadelphia, 1958. $4.00. A series of lectures sponsored by the Institute in March-May, 1958. D. C. Simons spoke on "Space Medicine—The Human Body in Space" (pp. 161-178). The problem is taken up, briefly, also, in I. M. Levitt's "Satellites and Travel in the Future."

PSYCHOLOGY AND HUMAN PERFORMANCE. By Robert M. Gagne and Edwin A. Fleishman. xiii + 493 pp. Holt, New York, 1959. $7.25. An introductory survey of the basic components of behavior, treating psychology as experimental science, with heavy reliance on graphic presentation of facts and emphasis on the interaction of the individual with his environment.

MEDICINE AND ANTHROPOLOGY. By Iago Galdston. 165 pp. The New York Academy of Medicine Lectures to the Laity, No. XXI. International Universities Press, New York, 1959. $3.00. A more precise title for this readable and useful collection of 6 lectures would be "Medicine and *Cultural* Anthropology" dealing as it does with such topics as acculturation, magical practices, and relations between medicine and humanities.

PHYSIOLOGICAL BASES OF PSYCHIATRY. Edited by Horsley Gantt, M. D. xiii + 344 pp. Thomas, Springfield, Illinois, 1958. $10.50. Articles presented at the 25th Anniversary of the Pavlovian Laboratory in Baltimore and at the first meeting of the Pavlovian Society held at the Behavior Farm Laboratory (H. S. Liddell, director). A bibliography of publications from the Pavlovian Laboratory of the Phipps Psychiatric Clinic, Johns Hopkins University, is usefully appended.

THE INEFFECTIVE SOLDIER: LESSONS FOR MANAGEMENT AND THE NATION. 1. The Lost Divisions (xx + 225 pp.) 2. Breakdown and Recovery (xvii + 284 pp.) 3. Patterns of Performance (xix + 340 pp.). By Eli Ginzberg and coworkers. Columbia University Press, New York, 1959. $6.00 per volume. Concerned with the personality and performance of young Americans who were rejected for military service during the second World War or who were prematurely discharged because they were judged to be deficient in the qualities that make good soldiers.

ANATOMY OF THE HUMAN BODY. By Henry Gray. 27th edition. Edited by Charles Mayo Goss. 1458 pp. Lea and Febiger, Philadelphia, 1959. $17.50. Few textbooks indeed, live to be one hundred years young: Here is an exception, the centennial edition of the justly famous *Gray's Anatomy*! The first American edition was brought out by Blanchard and Lea in Philadelphia in 1859, a year following the publication of the first English edition in London.

THE BASES OF SPEECH. By Giles Wilkeson Gray and Claude Merton Wise. xiii + 562 pp. Harper, New York, 1959. $6.00. A broad treatment of the nature and function of speech, including the biological (neurological, physiological) bases of speech.

PEDIATRIC METHODS AND STANDARDS. Edited by Fred H. Harvie. v + 330 pp. Lea and Febiger, Philadelphia, 1958. $4.50. The third edition (the 1951 and 1956 editions were intended only for local use) of a pocketsize *vade mecum* prepared by the staff of the Department of Pediatrics, School of Medicine, University of Philadelphia. Of special interest may be the chapters on Growth and Development (pp. 9-39).

THE INFLUENCE OF LIVING AND WORKING CONDITIONS ON HEALTH. International Medical Association for the Study of Living Conditions and Health, Vienna. xvi + 607 pp. With summaries of communications in English, French, German, Russian and Spanish. Proceedings of a conference, held at Cannes, (France) 27-29 September 1957. The three central topics were: role of nutrition in disease, effects of work on physical and mental health, and late effects of ionising radiation on man's health.

THE ONSET OF STUTTERING. By Wendell Johnson and Associates. ix + 276 pp. University of Minnesota Press, Minneapolis, 1959. $5.00. A detailed report of a series of three investigations, conducted between 1939 and 1957. A total of 246 children who stuttered, an equal number of controls, and the children's parents were studied by means of interviews, clinical observations, and test procedures.

ABNORMAL HEMOGLOBINS. By J. H. P. Jonxis and T. H. J. Huisman. 39 pp. Thomas, Springfield, Illinois, 1958. $2.00. The full-length title of this small publication is *Manual on Techniques for Studying Abnormal Hemoglobins.* "The question of the geographical distribution of different normal and aberrant forms of haemoglobin is, like that of the blood groups, of the greatest interest to physical anthropologists in its bearing on the interrelationships of different elements of the human population."

RESPIRATORY PHYSIOLOGY AND ITS CLINICAL APPLICATION. By John H. Knowles. ix + 256 pp. Harvard University Press, Cambridge, 1959. $5.25. Vital capacity is one of the few items of "quantitative functional anatomy" that has been used by physical anthropologists. The present volume provides a readable introduction to the study of lung volumes and to many other problems of respiratory physiology and pathology.

MALADIES HÉRÉDITAIRES DU MÉTABOLISME CHEZ L'ENFANT. By Maurice Lamy, Pierre Royer and Jean Frézal. 259 pp. Masson, Paris, 1959. 3,600 fr. The authors discuss in detail the inherited metabolic disorders of the child, a field in which substantial progress has been made in the last decade due to the pooling of the resources of biochemists, geneticists and clinicians.

ALCOHOL IN ITALIAN CULTURE. By Giorgio Lolli, Emilio Serianni, Grace M. Golder and P. Luzzatto-Fegiz. xv + 140 pp. The Free Press (and Yale

Center of Alcohol Studies), 1958, Glencoe, Illinois. $4.00. "For Italians drinking is a part of eating, even a form of eating, for wine is a food." This set of cultural traditions is considered at least partly responsible for their relative sobriety.

EXPERIMENTAL DESIGN IN PSYCHOLOGY AND THE MEDICAL SCIENCES. By A. E. Maxwell. 147 pp. Wiley, New York, 1958. $3.75. The material presents the content of lectures given to postgraduate students at the Institute of Psychiatry (Maudsley Hospital), University of London. The most frequently applicable experimental designs are presented in simple language using a minimum of mathematical terminology.

BIOCHEMISTRY AND THE CENTRAL NERVOUS SYSTEM. By Henry McIllwain. vii + 288 pp. Little, Brown, Boston, 1959. $10.00. The need for a second edition in a short span of 4 years and the fact that a quarter of the work quoted was published during this period bears eloquent witness to the rapid development of this field of inquiry.

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a record of research

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a record of research

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Review Editor

JOSEF BROŽEK

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